Antiquity

A Quarterly Review of Archæology

Edited by
O. G. S. Crawford, C.B.E., Litt.D., F.B.A.

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EDITORIAL NOTICES

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Antiquity

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Editorial Notes

'ANTIQUITY' is not a news-journal; a quarterly publication can seldom hope to be 'first with the news' when there must be so long an interval between getting the news and publishing it. Our function is rather to tell our readers about the most interesting things that are going on in the archaeological world; and also to foreshadow coming events of outstanding importance. In this number we publish two articles which are definitely 'news', being the first reports of new discoveries. Mrs Woolner describes some pictures of ships scratched hundreds or thousands of years ago on two standing stones of the Bronze Age temple of Hal Tarxien in Malta. The temple was deeply buried until it was rediscovered and excavated forty years ago by the late Sir Themistocles Zammit. During all those years the ship-graffiti were exposed to view and just as plainly visible as they are today; and the stones must have been looked at by thousands of people (including the present writer). But, like those axes carved on the stones of Stonehenge (illustrated in ANTIQUITY No. 109, March 1954), the graffiti escaped notice until Mrs Woolner spotted them. There may be other graffiti awaiting discovery both in Malta and elsewhere (perhaps in Sardinia, for instance?); but, of course, they can hardly be expected to survive on monuments that, unlike Hal Tarxien, have long been exposed to weathering. Mrs Woolner's interpretation of these ship-graffiti as thank-offerings of grateful mariners is supported by a telling modern parallel. If she is right it shows once more how little the substance of the Mediterranean religion has changed throughout the millennia.

It was during the middle and latter part of the 2nd millennium (say about 1500 to 1100 B.C.) that there was a great outburst of seafaring all over the ancient world, which modern opinion attributes largely to the Mycenaeans. We must not forget, however, that this was also the period of Egyptian imperial expansion, and that one of the chief evidences of far-flung sea voyages is the discovery of those segmented and other faïence beads which were chiefly made in Egypt about 1400 B.C. They have been found in Britain and Ireland (quite recently by the late Professor O'Riordain in the mound above a Passage Grave on the Hill of Tara); in France, Spain and Lipari, and in Greece. But they have also been found in India, whither Mycenaeans could never have gone by sea. In our next number we hope to publish an article on this fascinating subject of early shipping by Dr R. G. Barnett of the British Museum.

The other news-item we now print is the Note by Professor Marinatos on his discovery of an intact group of burials in a ruined tholos at Pylos on the west coast of Greece. Pylos is famous as the home of Homer's Nestor whose palace is being excavated by Professor Blegen. A tholos is a round building of dry stone walling, the stones being made to overlap, course by course, in the manner known as corbelling, so that ultimately they almost met at the top, thus forming a sort of dome over the tomb. Similar roughly contemporary tombs in Spain, Portugal, Brittany, Britain and Ireland are usually called Passage Graves; and although it is generally agreed that there must have been some cultural connection between the builders in the different regions, it is still not known exactly what form it took. The new discoveries (illustrated on Plates IV to VIII) are of outstanding artistic excellence, some of them recalling the inlaid daggers of Mycenae.

Jericho needs no apology to our readers who will by now have realized that it is opening up a new world, hitherto unsuspected. Our present instalment is a friendly argument between Professor Braidwood who is excavating what we must perhaps call a rival site at Jarmo in Iraq, and Dr Kenyon, who has boldly gone back to prepare the fifth season's excavations at Jericho. Such arguments are normal and necessary in all scientific work, and are a sign of vitality. Knowledge is advanced by informed criticism as well as by new discoveries, and Jericho has provided both. This truth—or truism—is often misunderstood by people unfamiliar with the atmosphere in which modern archaeologists work and live ; whatever may have happened a century ago, before archaeology became a science and a profession, it is now wholly untrue to say, as did the Minister of Works recently in the House of Commons, that ' there is a profound suspicion among all archaeologists about what all other archaeologists say or do '. Archaeologists are sometimes critical of things done or left undone by his Ministry, particularly at Stonehenge, where a recent instance could be given of expert assistance foolishly refused; and they rightly expect that, before such major operations as the raising of fallen stones—itself a perfectly, proper but ticklish undertaking—are begun, there shall be an assurance of full and genuine co-operation with experts outside the Ministry. We only become ' suspicious ' when we are informed in political communiqués that there is ' complete identity of views on all the subjects under discussion ', because the facts are always obviously otherwise. People who live in glass houses should not throw stones.

Professor Vogt's article is an excellent instance of this ' informed criticism '. By using the modern most careful excavation technique he has proved that the lakeside settlements of Switzerland, formerly thought to have been built on piles over the water of the lakes, were in fact built above damp, but not watery, ground. The opinion that they were ' lake-dwellings ' was based upon faulty observation and grossly deficient methods, in the days before archaeological excavation-technique was developed, that is, in archaeology's pre-scientific days. Here, if anywhere, has critical observation advanced knowledge. Mr Gabel's article, too, criticizes earlier views about the Neolithic Period in Europe, but from a different angle.

It is hard for the Editor to maintain a balance between light and heavy matter in each number; it should be added that these adjectives convey no disparagement whatever; for in this kind of journalism light articles often outweigh heavy ones! We hope our readers

will agree that in this present number the balance is kept. Our next number will contain, besides the article already mentioned on early shipping, a very important one on a subject that cannot be disclosed beforehand, and another on Balkh, mother of cities, in Afghanistan by Mr Allchin. It is good news that Sir Mortimer Wheeler is organizing an excavation there. Balkh is a vast site; the town itself was seven miles in circumference, and it is perhaps the most outstanding site yet remaining virtually unexcavated. Our December number will be devoted to Evolution, in anticipation of the centenary, in 1958, of the first announcement of Darwin's theory of the Origin of Species. Articles will deal with the evolution of Man himself, of his tools and of society.

We offer a hearty welcome to the first number of *Scottish Studies* (published twice yearly for the School of Scottish Studies by Oliver and Boyd of Edinburgh, £1 a year). The School was founded in 1950, with the blessing of the University of Edinburgh and the (perhaps more effective) ' generous aid of the Carnegie Trust '. Its activities cover archaeology (especially in the direction of compiling maps of prehistoric and early historic Scotland), place-names, oral traditions, folklore and music. The School has a local habitation and a staff, and as already a going concern it deserves support and encouragement from Scotsmen all over the world, who might well profit themselves and help the School by subscribing to *Scottish Studies*. This number contains 150 pages and many illustrations. The articles deal with the School and its works; historical contacts between Scotland and Scandinavia; Scottish cadastral maps; folk-tales; the plough in Scotland; and the classification of Gaelic folk-song. The article on maps deals with some fascinating old 18th-century Estate Plans in Lothian, illustrated by copies beautifully drawn by Miss Findlay, and also by air-photographs of the same areas. The only criticism we have is of the format, which could be improved, e.g. by condensing the prelims, for which four pages seem unnecessary, by adding page-numbers to the List of Contents, and in various other ways.

ANTIQUITY XXXI, 1957

Graffiti of Ships at Tarxien, Malta *

by DIANA WOOLNER

IN May 1956, while visiting the megalithic temples of Tarxien, which stand on high ground less than a mile from the inner creeks of Grand Harbour, massed graffiti of ships were observed on two adjacent uprights in the ' Third Temple '.

The Tarxien temple group was excavated in 1915–9 by Sir Themistocles Zammit, to whose son Mr Charles Zammit, Director of the Museum Department of the Maltese Government, the discovery of the graffiti was reported; he confirmed that they had hitherto passed unnoticed. His ready co-operation and his generous provision of the photographs illustrating this article are gratefully acknowledged, as is also the great interest shown, and help given, by Mr R. S. Newall, who happened to be visiting Malta at the time of the discovery.

The Maltese temple plan is an oval; it has an extension, typically apsidal, opposite the main entrance on the line of the short axis produced. The more developed temples, of which Tarxien is the most elaborate, have a second adjacent oval. Thus the extension, or sanctuary, is approached across two nearly rectangular courts, each having two lateral apsidal chambers partitioned from it. These chambers are often termed ' chapels '. The two stones bearing the graffiti form the further side of the entrance to the first chapel on the left entering the ' Third Temple '. The chapel, which in plan appears to justify this term, contained the most sophisticated carving surviving at Tarxien, including the relief friezes of animals such as are known to have been sacrificed in the temple. The entrance to the chapel is flanked externally by two blocks carved with spiral ornaments. These decorations are, according to Ashby, all of the latest temple period (1). The excavation of these temples is described by Sir Themistocles Zammit in his *Prehistoric Malta, the Tarxien Temples* (2) and in his reports in *Archaeologia* (3), where plans and photographs show the entrance and the two stones *in situ*.

The entrance is 2 ft. 6 in. wide and 4 ft. 8 in. long and has a neatly cut threshold 3 in. high and 1 ft. across. Only part of the two orthostats forming one side of the entrance, and the stumps of those forming the chapel, remain. The carved blocks flanking the entrance were found *in situ* and buried in about 3 ft. of silt which lay throughout most of the temple, sealing the original floor level from all subsequent periods. Above the silt the ground had been made up with broken stone as a foundation for the field earth when the mound covering the temples was levelled for cultivation. This is the customary Maltese practice. The ground in the chapel had been disturbed and there had been quarrying at some time. No folk-memory of the buried temples seemingly survived, nor were they known to Malta's early antiquaries.

The stones bearing the graffiti are of the local soft, honey-coloured limestone, containing harder fossils. This stone is easily worked and, although it hardens on exposure to the air, it weathers badly. The two stones now measure: (A) about 5 ft. × 2 ft. × 9 in. and (B) about 5 ft. × 2 ft. 7 in. × 1 ft. 2 in.; both taper slightly towards the top. The

* Published by permission of the Minister of Education in the Maltese Government.

faces were finally dressed after erection by rubbing, and 1 to 2 in. have been left at the foot of Stone A forming part of the pavement. This stone is also dressed on the edge facing the chapel, and on this edge are two pairs of ' rope-holes ' about 3 ft. apart. These ' rope-holes ' are characteristic of the Maltese temples and, although their purpose is uncertain, they here suggest hinges for a light door. The back of Stone A is dressed for about 5 in. along the edge to within 2½ ft. of the ground. The lower 2½ ft. of this stone, back and front, are badly weathered, light-coloured and friable. Stone B is butted close against Stone A; it is dressed on the edge facing the central court, and also roughly dressed on the back for a depth of about 1 ft. at the top to about 6 in. towards the bottom. The lower part of the face of this stone is weathered in a similar manner to Stone A but reaching a height of 3 ft. 9 in. These weathered parts are those which were buried in the fine silt, and have deteriorated since re-exposure to the air. The tops of both stones were hacked off when the field was levelled. The pick marks and fractures appear modern but not recent. The top 3 in. of Stone A, and up to 5 in. of Stone B are also weathered, but hard and grey, and are presumably those parts exposed during cultivation. The best-preserved surfaces seem to be those that were embedded in the broken stones. The graffiti, which only occur on the dressed surfaces, begin about 2 ft. 6 in. from the ground, becoming more numerous between 3 ft. 6 in. and 4 ft., and continuing right to the present tops of both stones, and evidently continued higher. There are some graffiti on the edge of Stone B facing the central court, beginning at 3 ft. 8 in. from the ground and continuing to the broken top. On the back of the same stone are two or three faint vestiges of graffiti at a similar level (PLATES I–III).

There are about three dozen figures, mostly identifiable as ships of divers types drawn in the manner of divers people. Even the unidentified figures suggest ships or parts of ships. There are no other matters represented. Many drawings are superimposed on others. This usual phenomenon of graffiti is partly explained by a new graffito showing white and clear on the fast weathering stone irrespective of those already there, which could easily be ignored by the draughtsman. All are crudely drawn. They are the only example of their kind known in Malta, and comparative material is unsatisfactory and scarce, but an attempt has been made to point out such similarities as have been recognized.

There are graffiti of ships believed to be *ex voto* on churches both in Malta and Gozo, but none can be earlier than the 16th century, and in all such the familiar lateen sails of Mediterranean ships in historic times is a characteristic feature. This is nowhere suggested at Tarxien; nor is there any suggestion of the ram prow, that distinctive feature of ancient warships.

The only comparable group of graffiti known at present is reported from Japan in a megalithic passage grave (4). The mass of superimposed ship graffiti is strangely similar, although the forms of course differ. The coincidence is too remarkable to pass unnoticed.

There is one basic type of ship graffito which is very widely distributed both in time and space. It consists of a crude hull line above which are drawn a number of vertical lines. Such representations, with variations, are recorded from Scandinavia, the Canary Islands, Easter Island, Victoria Nyanza, the Middle Nile region and the Cyclades. This type occurs frequently on the Tarxien stones.

Different methods have been used to produce the graffiti. Pounding, chipping, scraping and scratching occur. Different tools were used, the most remarkable being those with a double or multi-point or serrated edge. Where single, double or multiple lines occur in the same figure it is probable that they were made by the same tool held at a different angle. The tools most likely to produce these effects are the fossil sharks' teeth which abound in Malta and are commonly found on ancient sites.

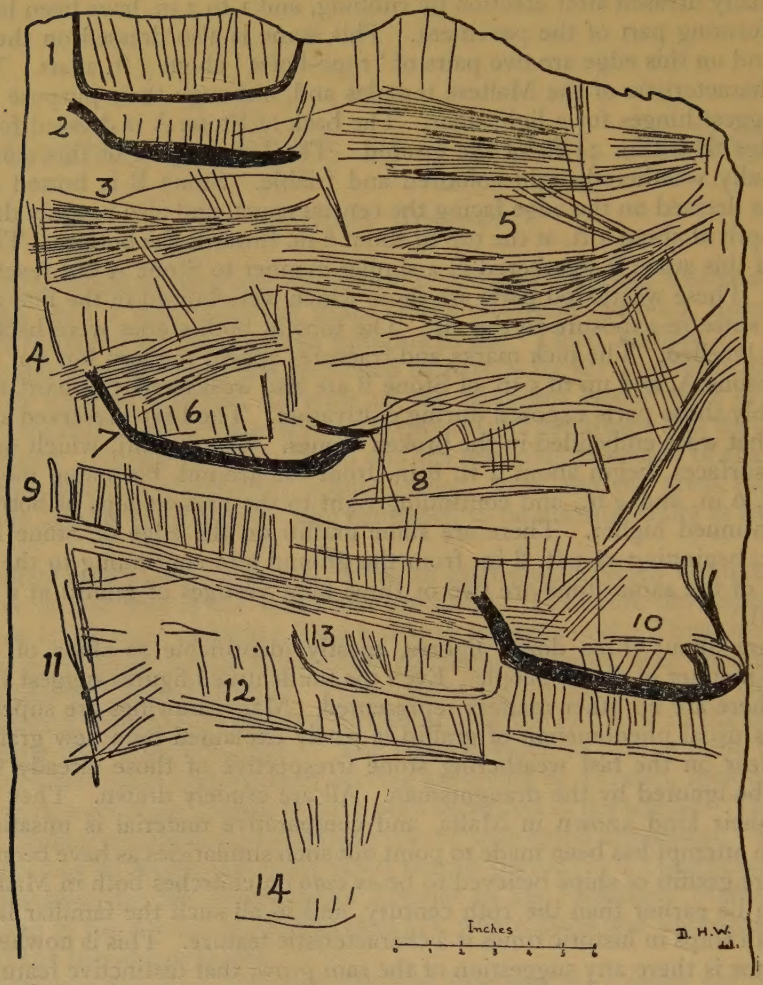

FIG. 1. STONE A, HAL TARXIEN.

INTERPRETATION OF THE GRAFFITI

(The numbers refer to the figures in the diagrams)

Stone A (PLATE II)

1. This is of the basic type to which nine or more other figures have been assigned. All are drawn with a single fine point, though some are blurred by weathering. The figures of this type are: 13, 14, 15, 18, 19, 24, 37, 39 and 40.

2. The hull is deeply engraved with a coarse point, although the vertical lines suggest the basic type; the hull line is well developed and the stern appears to be surmounted by an ornament. Cf. FIGS. 7 and 10; see 10, *infra*.

3. The outline is drawn with a fine point and filled in by scratching with a double point. This figure remains undeciphered.

4. Drawn with a fine point and filled in by scraping, perhaps with the side of the tool. This figure also remains undeciphered and may be connected with 3. It underlies 7.

5. Strongly scratched with a coarse point. A double point has been used for filling in and for the sheets. This is a typical Egyptian wooden ship, familiar from the tomb models. The sail is shown partly furled. The sheets and perhaps the steering oar are indicated. Further details were shown in the stern but have been destroyed. The bows are confused in 3 and 4. This ship, though badly drawn, is recognizably in the Egyptian convention.

6. Lightly drawn and faint. Underlies 7; see 10, *infra*.

7. The well-formed hull is deeply engraved with a double point. It suggests the ' Bull's Horns ' profile of ships in the *Iliad*. There are a few diagonal lines in the ship, and perhaps a deck line. The steering oar is probably shown, but confused with 8. There is a long unexplained process issuing over the stern and obscured by a quantity of undeciphered lines. It overlies 4 and 6. For the bows cf. FIGS. 2 and 10; see 10, *infra*.

8. Lightly scratched and confused by many later undeciphered lines; see 10, *infra*.

9. This is one of the most significant ships. It is firmly drawn and stretches right across the stone. A broad double point has been used for the hull, the lower part of the bows and many of the vertical lines; also for a long horizontal line drawn through and beyond the forepart of the ship which may, or may not, be related to it. This drawing has remarkable affinities with the ships depicted in early Cycladic pottery, and appears to be drawn in that convention. Observe the long, nearly straight hull, the very high prow rising at a sharp angle and surmounted by some ornament, and the many vertical lines along the hull, their tops joined by a horizontal line. These and the general proportion and style of the graffito all have their counterparts on the Cycladic pottery. There is, however, no horizontal projection at the stern as in the Cycladic ships, but a short vertical stern distinct from the internal vertical lines. This graffito underlines 10; the other graffiti resembling it in form are 33 and perhaps 21, 25 and 27. All these except 33, which is on the edge of Stone B, head to the right as do the majority on the Cycladic pottery.

10. The hull is deeply engraved, which makes this, with 6 and 2, the only other ships rendered in this manner, the most noticeable graffiti on the stone. A many-pointed instrument was used for the hull, the stern and deck line; a double point for the bows and the stern ornament, and a single point for the internal diagonal lines and the ' oars '. This figure overlies 9.

The stern curves right over the ship in the Greek manner, and is supported by a brace. This feature is found in early Greek vase painting and also occurs on a Cretan seal. The stern ornament is very like the ' plume ', and the general line of the hull not incompatible with that of the merchantmen familiar from Greek vases. Although not identical in form, FIGS. 2 and 7 must be classed with 10 on the grounds of style. All three head to the left.

Below the ship, and apparently dependent from it, are nearly vertical lines joined by horizontal lines at the bottom. It is possible that these represent twenty oars; there are nineteen strokes and one gap. The blades of the oars, or their point of entry into the water, were sometimes represented by horizontal lines, as on Sidonian coins. If this part of the graffito be accepted as the oars of 10, then it may well be that FIGS. 6, 8, 11, 12 and perhaps 26 represent banks of oars drawn without ships. This suggestion is without precedent and is only tentative. If the lower part of 10 is not meant for the oars it can only be another ship of the basic type together with the five other figures already mentioned. It should be noted that of the five ' banks of oars ' two show the use of a double point, whereas in the ten or more figures ascribed to the basic type no double point has been used, and all show a slightly curved hull line in contrast to the angular form of the ' banks of oars '.

11. Lightly scratched. A double point was used for most of the figure which appears to be drawn on end to fit an empty space. If so it is later than 9 and 12; see 10, *supra*.

12. Scratched with a sharp point, a double point being used for the external lines of the figure; see 10, *supra.*

13. Lightly scratched. One or perhaps two ships of the basic type; see 1, *supra.*

14. Faintly scratched. Traces of one or more ships of basic type; see 1, *supra.*

Stone B (PLATE III)

15. Faint and very weathered, one ship of basic type and traces of another above it; see 1, *supra.*

16. Traces only of a ship lost by weathering. A fine double point used.

17. A fine double point used for the lines, filled in by chipping as if with a small chisel. The form is confused with 20 and there may be several superimposed drawings here.

18. Lightly scratched; see 1, *supra.*

19. Ditto.

20. Strongly incised with a coarse point, the outline filled in with lines made with the same tool. This appears to be an Egyptian spoon-shaped ship with the typical vertical stem familiar in Egyptian art. The stern is lost in 17. This ship is distinctly in the Egyptian convention, although it should be noted that the Egyptians represented Phoenician ships as of this form also.

21. The base line is strongly incised right across the stone, the bows being lightly scratched and filled in with lighter lines. This may have been a ship similar to 9 on which 20, 22 and 23 have been superimposed. It appears to overlie 24; see 9, *supra.*

22. A coarse point used, and the form filled in with heavily incised downward curving lines. There is a pointed bow (or stern), the other end being lost in 20. It overlies 23 and 24.

23. Lightly drawn with a double point, the hull line is faint, and there may be a bank of ten oars, eight strokes with two gaps; see 10, *supra.* Above 19 is a mass of horizontal lines and one long vertical line which suggest a mast and sail belonging to 23. This figure underlies 22.

24. Faintly drawn. Of the basic type, it is apparently later than 25, 27 and 28, but it seems to underlie 20, 21, 22 and 23; see 1, *supra.*

25. Faint and weathered; see 9, *supra.* This figure overlies 26 but is earlier than 24.

26. Drawn with many scratches of a fine point. Perhaps this figure should be classed with 9, *supra,* but see also 10, *supra.* It is earlier than 25 and 27 but overlies 28.

27. Drawn with many scratches. Only the bows of a ship of the type of 9, q.v., *supra.* The stern is lost in a bad patch of weathering. It is earlier than 24 but later than 26.

28. Pounded into the stone and in part badly weathered. This U-shaped depression might represent a ship in a primitive convention, such as those painted on pre-dynastic pots, and merchant ships drawn in the Assyrian idiom are almost of this shape; but it seems more likely than it has nothing to do with the graffiti but was already on the stone; however, cf. 35. It is certainly earlier than 24 and 26. This figure is visible in a photograph taken during the excavations ((3) Pl. xv).

29. Finely drawn with a single point for the vertical lines and a double point for the horizontal lines; the outline is strongly incised. The artistic convention is less primitive and the drawing more careful than in any of the other graffiti. An attempt appears to have been made to show the construction, but most unfortunately most of this figure has been destroyed by weathering, only the bows remaining; this is perhaps surmounted by a tall flagstaff. Part of 30 may be the stern of this important ship.

30. Weathered and faint. Part of this may represent the stern of 29, but there may also be the remains of a ship of basic type; see 1, *supra.*

31. Drawn with a fine double point, but weathered and faint. This might be a rough example of the type of 29.

32. Very weathered vestiges of one or more ships of the basic type.

Edge of Stone B

33. This graffito was produced by a method different from any of the others. The shape was pounded into the stone and the lines of the hull and high prow then incised with a coarse point in the depression. The stern has been broken off with the top of the stone but the spring of the upward tilt remains; see 9, *supra*. This ship is clearly visible in the photographs taken during the excavations ((2) Pl. II and (3) Pl. XVI).

34. Deeply incised with a coarse point, these long curved lines may represent an unfinished ship. They are visible in a photograph taken during the excavations (*ibid.*). Overlies 35.

35. Pounded and very rough. This small ship has a nearly perpendicular stem and stern, but see 28, *supra*. It underlies 34.

36. Scratched with a fine point, but otherwise similar to 34.

37. Faint traces of a ship of basic type; there may have been another below it.

38. Pounded figure, possibly the prow or stern of an uncompleted ship, but see 28, *supra*.

39 and 40. On the back of Stone B are at least two very faint and weathered examples of the basic type. See 1, *supra*.

The date of these graffiti cannot be considered apart from the difficult question of that of Maltese prehistoric culture as a whole. It can, however, be demonstrated that they are probably contemporary with the latest temple period. There is now general agreement that the Tarxien temple group was abandoned about the middle of the 2nd millennium, and that the animal friezes and spiral ornaments associated with the stones bearing the graffiti belong to the final phase. It has been suggested that the greater refinement of this period was due to Malta's first direct contacts with the Eastern Mediterranean culture, and renewed Cycladic influences (5) (6). The ships represented in the graffiti give support to this theory.

A cremation cemetery was found immediately above the layer of silt which had accumulated on the temple floors, and this has been shown by Evans to be before the 14th century B.C. (5). Although these burials did not extend over the whole temple area they determined the end of the uninterrupted accumulation of silt. This silt, known to be undisturbed in the court and sealed by the cremation cemetery layer, covered the spirally ornamented stones flanking the entrance to the chapel, and must have extended through the doorway. How far it remained undisturbed in the chapel is unrecorded. Potsherds and fragments of ornamented stones, all apparently of the temple period, were found in and near the entrance (7).

A little research on the walls in the public places of Malta has revealed the normal distribution of graffiti today. They begin at between 2 ft. 6 in. and 3 ft. from the ground and increase in density up to a maximum at between 4 ft. and 5 ft.; they then decrease for another foot. Now this is just what is found at Tarxien, supposing a man of normal Mediterranean stature to be standing on the original temple floor. The graffiti could hardly have been drawn with their significant distribution on the stones after the accumulation of silt and debris. The opinion is therefore offered that these rough sketches—the only known prehistoric representations of ships in these remote islands of the ancient

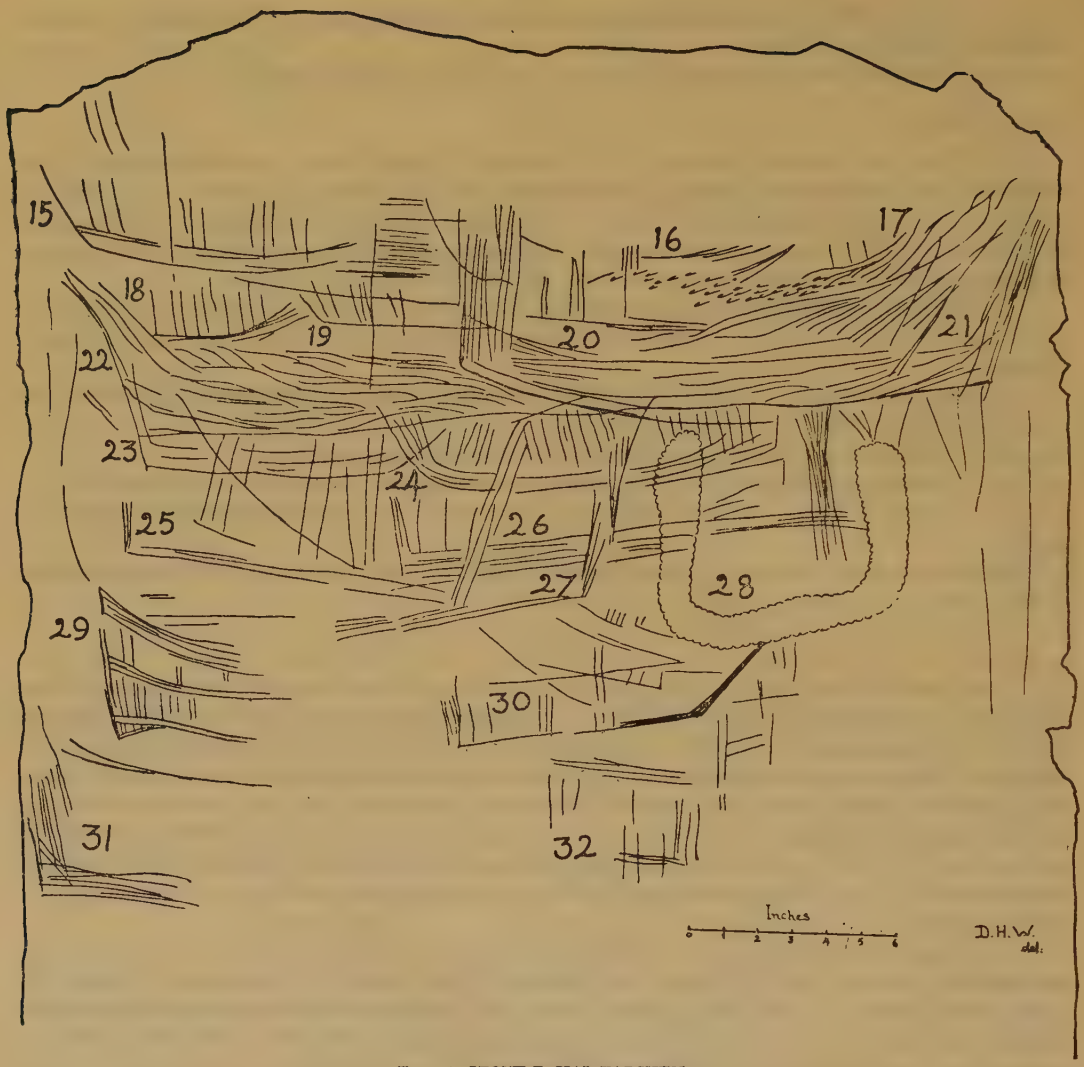

FIG. 2. STONE B, HAL TARXIEN.

world [1]—were scratched by mariners in the middle of the 2nd millennium B.C. shortly before the abandonment of the Tarxien temples.

One possible explanation of the purpose of these graffiti springs unbidden to the mind. As Gordon Childe suggests (8), the natural harbours of Malta offer a refuge to mariners driven off their course; the autumnal gales, the ' great wind Euroclydon ', blows each year under the significant name Gregale, and makes the circumstances of St Paul's shipwreck (9)

[1] A gold gemel ring found in a rock tomb in Malta was published by Sir Themistocles Zammit in the *Antiquaries Journal*, July 1925, vol. v, no. 3. Engraved on the bezel is a typical small Greek galley of *c.* 500 B.C. The two halves of the bezel were made with interlocking teeth which were mistaken by Zammit for oarsmen and led him to misinterpret the galley as a ship of the basic type and therefore of early date.

very clear to anyone who has seen its fury. The story may serve as an example of what must have befallen earlier seafarers who ventured too late in the season. The custom of *ex voto* offerings in the form of pictures is well attested in the ancient world (10), and such may still be seen in the churches of Malta today.

It is therefore suggested that these graffiti of divers ships of divers peoples are the votives of those who escaped from storm and shipwreck, and who, reaching shelter in the prehistoric backwater of Grand Harbour, paid their vows to some Deity, unknown to us, in the temples of Tarxien.

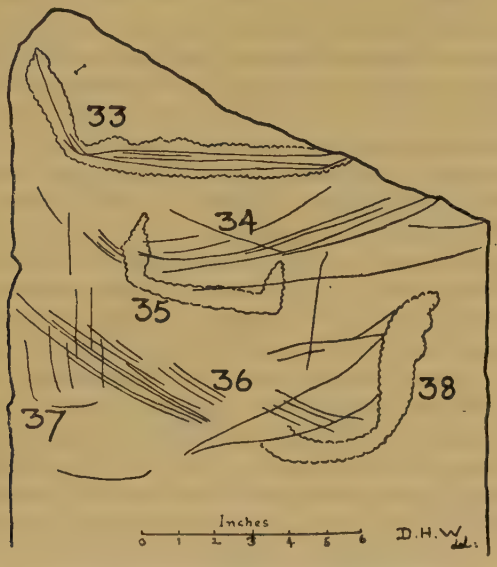

FIG. 3. EDGE OF STONE B, HAL TARXIEN.

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ANTIQUITY XXXI, 1957

Swiss Pile-dwellings *

by E. Vogt

FOR about a century it has been assumed that during the Neolithic and part of the Bronze Age the inhabitants of what is now Switzerland built pile-dwellings on the lakes. This interpretation of the finds was put forward, as is well known, by Dr F. Keller, who did so much for Swiss prehistory. His theory was based upon a comparison of the very scanty remains of prehistoric buildings in Switzerland with pile-dwellings that are to be seen today in many parts of the world, erected both on dry land and also on the shores of lakes and of the sea, standing in water. In spite of some opposition, Keller's interpretation appeared so convincing that it aroused great interest and was generally accepted by archaeologists. It was not accidental that Keller had one of his important books on pile-dwellings published in England. Every museum in the world endeavoured to obtain some specimens of the Swiss finds, and today it is practically impossible to make even an approximately complete list of the older finds from the pile-dwellings.

It is rather unfortunate that the habitations on the shores of the Swiss lakes were discovered so long ago, for then no proper technique of excavation existed. It was not then thought necessary to record observations and the results of excavations on plans so as to provide an adequate survey of a habitation site or part of it. Prehistoric research was in its infancy, and it is therefore not surprising that Keller did not think it necessary to publish a scientific account substantiating his theories. The mere facts that occupation layers and posts were found in the water, and that it seemed impossible for the occupiers to have lived in amongst that closely set array of posts, were enough at that date to make Keller's interpretation seem convincing. It was also assumed that the fluctuations in the water level were the same then as today. Further, it was stressed that the Lake Marl often found beneath the occupation layers was an unsuitable foundation for houses. If one looks today in the older publications for documentary evidence that could be used for scientific purposes one soon finds that none exists. It cannot, therefore, be assumed that the theory of pile-dwellings standing in water was based upon scientific observations carried out in accordance with modern standards. Proper investigations of pile-dwellings began only with the adoption of modern methods of excavation, that is, in the present century. For some time past there has been a noticeable uncertainty in the views expressed about pile-dwellings, replacing the former unanimity in accepting Keller's interpretation. Those concerned with their excavation have to make a fresh start in tackling what is in fact a most complex problem. New excavations have become imperative; and during the last few years several Swiss sites have been dug.

One important mistake made in the past has been the assumption that the problem could be solved exclusively by a study of the settlements on the lake shores. Actually it must be tackled as merely one aspect of the whole subject of prehistoric settlements. People belonging to the same cultural group built ordinary settlements as well as pile-dwellings, and there are also bog settlements. Furthermore, we need to take into account those settlements which grew up on marshy ground during other periods. Though attempts have been made for many years to solve these problems, they have often been

* Translated by Dr Maria Bersu and the Editor.

made with insufficient means and from a wrong point of view. Archaeologists have got to collaborate with their colleagues in other branches of science, and it will be some time before answers are found to all the questions. However, it can already be stated that the lake settlements are not such extraordinary structures as has hitherto been believed, though compared with others, past and present, on dry ground they do present a special case.

The author of this article has published an account of recent archaeological and other scientific evidence about pile-dwellings in a book called *Das Pfahlbauproblem* (1955). In it he has set forth the evidence, which, as he thinks, disproves Keller's theory; and, since its publication, important fresh evidence has been revealed by the excavation of a new settlement discovered at Egolzwil in the Canton of Luzern.

One of the fundamental problems of Neolithic and Bronze Age settlements is that of the soil on which the houses were built. Besides settlements on hard ground we have others on loose, unstable soil, dating from various periods. To the Mesolithic period belong those of Star Carr in England, Ulkestrup in Denmark, and Duvensee in Germany. There were many such during the Neolithic and Bronze Ages. They are found again during the Iron Age at Glastonbury in England and during the Middle Ages in Ireland, where they are called crannogs; and many more will certainly be discovered. The soil on which they are built varies considerably. Often they stand on peat, in bogs and on the boggy shores of lakes. On lake shores the ground is very often Lake Marl (gyttja and seekreide); more rarely it is formed of sand or some other kind of sediment. As these soils differ widely in their composition, it is only to be expected that we find a difference in the technical details of the structures. Moreover, at Egolzwil it was found that a sharp distinction could not always be drawn between the shore and the bog. It could be proved that the surface of the Lake Marl below the habitation layer was covered by faint traces of vegetation. It seems, therefore, that the process by which the shore was converted into a bog was a gradual one, even if it was not actually covered by water from a rise in the lake level.

The fine state of preservation of wooden implements in many bog settlements and pile-dwellings proves that the habitation layers and floors must always have been damp. In several settlements wood belonging to the earlier as well as to the latest structure has been preserved, proving that the floor cannot have been dry at any time during the occupation. It is interesting to observe how the inhabitants coped with this dampness of the ground. In the bogs protection was relatively easy; there were nearly always wooden floors. The carrying capacity of the bog seems to have been favourable for this technique. Yet during the last excavation at Egolzwil IV we found that, besides houses with wooden floors, there were also some without such floors. The habitation level consisted simply of peaty matter, strips of bark and occupational debris. The methods of constructing the wooden floors differed widely. In the most primitive buildings layers of sticks were placed so as to lie directly upon the surface of the peat. In others long beams were laid side by side and covered crosswise by planks. But the insulation thus secured lasted only for a short time, as the planks were soon pressed down into the ground. In the more elaborate structures, however, the insulation was better; for there were three layers of beams beneath the floor. This was the practice at Taubried in the Federseemoor, at Pfyn in the Canton of Thurgau and several other settlements. A singular case is that of House 10 in the bog settlement at Aichbühl, where, besides beam substructures, forked posts supported the topmost layer of beams. Another singular case occurred in the bog settlement of Riedschachen in the Federseemoor, where the insulation of the floor was obtained merely by means of forked posts, though the space between the floor and the ground was no wider than in some of the houses with beams. The object aimed at was not protection from flooding but insulation from the dampness of the ground. We should note that these floor

constructions differ widely from those of houses built on hard ground, where wooden floors were usually unnecessary.

What do we know about the floors of pile-dwellings proper (Pfahlbauten)? First of all it must be said that observations cannot be made on every site because we have to allow for the possibility of later destruction or of a shifting of the habitation layer. I will confine my remarks to two examples. In Egolzwil III the habitation layer was sandwiched between two layers of Lake Marl, a position which has always been regarded as typical of lake dwellings. A sediment deposited in water underlay the habitation layer, having been formed before the pile-dwelling was constructed. In Egolzwil III the habitation layer contained a large number of pieces of bark, sometimes as long as three metres, resting directly upon the Lake Marl; and they had been intentionally stripped from the tree-trunks. They were always lying flat and not as they would be if they had fallen into the water from houses. They nearly always lay with the inner surface downwards, so placed to prevent the bark curling. It follows that they were placed there so as to form an insulation layer on the ground. Such layers have been found on mesolithic sites at Ulkestrup in Amose (Denmark), at Duvensee (North Germany) and recently elsewhere. They often occur also on Neolithic plank floors. It seems strange today that wooden floors were not used on Lake Marl, nor is it clear why they were not; probably there were some technical objections. Reinerth thought he had found the remains of such wooden floors at Sipplingen on Lake Constance in a ' Pfahlbau ' permanently under water to-day. That inference was the result of his idea that the houses stood on short piles because of the danger of flooding. In fact there were well-defined clay hut-floors with traces of the party walls and entrances still visible. These floors could not, therefore, have been in a secondary position unless a complete wooden floor existed underneath. But below the clay layer were found only a very few wooden sticks without any clear arrangement. Reinerth assumed that the greater part of the wooden floors had rotten away. But during these excavations various wooden tools were found which should have perished much more quickly than stout wooden beams. The explanation is that the clay floors were placed directly upon dry ground. This agrees well with Reinerth's observation that there was no habitation layer beneath these clay floors—a fact which would not be possible in a pile-dwelling. For special reasons the people living at Sipplingen undoubtedly did not make wooden floors, but laid down clay floors as a protection against damp.

If any doubt should still remain about the conclusion that shore settlements, hitherto called ' lake dwellings ' (Pfahlbauten), were built away from the water on damp ground, it will be removed by the fact that everywhere at Egolzwil III primitive hearths were found on the habitation layer. They consisted of several irregular layers of clay with bits of bark and traces of habitation debris interposed between them; they must therefore have been remade several times. Having no stone paving their shape while in use must have become quite irregular. The older layers, pressed down by the later ones, sank into the Lake Marl and the whole came eventually to have a lens-shaped section just like that of the constantly renewed floors of the huts in the Glastonbury lake village. The hearths had to be continually remade as the habitation layer formed inside and outside the houses. We found the same thing in 1954 during the excavation of Egolzwil IV. The builders of what is called a *Moorhaus*, whose wooden floor was perfectly preserved, knew for certain that the weight of the hearth would bring about a sinking of the floor. They therefore made the hearth in an open space in the floor, placing it directly upon the ground below. In spite of this precaution the floor had to be renewed several times, and therefore the hearth as well. The various layers of the hearth eventually attained a thickness of one metre, and even here the section became lens-shaped. There cannot be the slightest doubt that the piles of clay at

Egolzwil were originally hearths and were still *in situ*; and the same remark applies to several other settlements. We conclude therefore that settlements hitherto regarded as pile-dwellings were certainly built upon flat ground.

The idea that these shore settlements were pile-dwellings certainly originated from the fact that there are such vast numbers of piles still sticking out of the water or the ground. Often they are so closely set together that it would have been impossible to live between them. An explanation of this fact was found at Egolzwil III, but it must be emphasized that the conditions at each site must be dealt with individually. At Egolzwil III we found that the piles projecting from the habitation layer must originally have been fixed in the soil below that layer. They have been driven down into the harder layers. After the abandonment of the settlement the parts of the piles exposed to water or air perished through corrosion. The older sediments (Lake Marl and gyttja) were constantly being depressed by the water pressure during periods of high water-level and subsequently by that of the Lake Marl and peat. Consequently the habitation layer sank to a much lower level, while the still existent portions of the piles did not alter their position. In other words, all the numerous piles now projecting from the habitation layer at Egolzwil were originally below (and covered by) the habitation layer and the structural portions—forming walls and roof—no longer exist. This forest of piles, therefore, is just the sum of all the piles in use in all stages of the settlement's existence, and they survive only because the stumps all remained embedded in the ground. It was almost impossible to pull out the pointed piles, three or four metres long, from the lake marl. If, bearing this in mind, we make a plan of the piles, we get exactly the same result as from the plan of a settlement of several stages or periods standing upon hard ground. The post-holes are set so close together that the posts obviously could not all have belonged to one period of building, or to a single building on ground-floor level. Now there is every indication that usually the shore settlements must have had several phases or stages of development. This means that all those piles have got to be allocated to successive stages of building. I have pointed out in my book that, under favourable conditions, we can sometimes unravel from the remains of shore settlements house-plans exactly corresponding to those of settlements on hard ground. It can therefore be proved that there is no essential difference between the shore settlements and those on hard ground except in the measures adopted (and described above) to guard against damp. These involved the adoption of certain technical devices—simple or complicated in proportion to the technical skill of the builders.

An argument regularly put forward by the champions of the pile-dwellings theory is the existence of so-called bridges or gangways. It should be stated, however, that up to twenty years ago not a single plan of any of these ' bridges ' existed. Well known are the two ' bridges ' of one of the settlements at Lüscherz, on the lake of Biel, which have three rows of posts. Though in several instances three posts appear to form the support of a bridge, it cannot be proved that they really did perform that function. Similar arrangements of posts have been found in timber trackways, and there is also a particularly good example in the Viking settlement of Trelleborg in Denmark. This argument in favour of pile-dwellings therefore falls to the ground.

There are many points which cannot be discussed here, but those mentioned prove that not much is left of the old theory of pile-dwellings. Prehistoric man in Switzerland evidently did not change the type of his habitations more than was made necessary by the environment. Besides the bog and shore settlements were those built on hard ground. The vital question is—what caused these people to settle on damp ground ? There must have been some advantages that more than compensated for the drawbacks. For an explanation we have to look outside Switzerland. It can be stated that the farmers of the older Neolithic

period, called the Bandkeramik culture, did not settle in the subalpine regions, with the exception of the Loess region. The reason must be that these subalpine regions were covered by forests both thicker and of a different kind, and that the glacial gravels on which they grew did not seem so attractive as the easily cultivable Loess soil. Unforested tracts were to be found on the dry mountain slopes, but they occurred also on the bogs and on the boggy banks of rivers and on the shores of lakes. Such regions must have been regarded as suitable for habitation sites for the reason that no trees had first to be cut down in order to make room for large villages. In order to have an area for cultivation it was nevertheless necessary to clear the ground, involving hard work that was avoided whenever possible. Habitations on lake shores imply that one had to settle in the rather extensive boggy zones alongside those shores. There may have been other reasons for choosing those sites for settlement, but we cannot discover them by archaeological means.

We can now see why this type of settlement is found occurring in many different kinds of Neolithic, and even in Mesolithic and Bronze Age cultures. It was dictated by the terrain, not by the state of culture. The transition from settlements on hard dry ground to others on boggy ground, or even to some on not very hard and sparsely overgrown shores, is in any case less revolutionary than would be a move to settlements on piles standing in water, which would have been a very complicated and difficult undertaking. In this matter a comparison with modern pile-dwellings is to no purpose. Prehistoric European settlements must be explained from the actual remains themselves interpreted by archaeology and other branches of science.

ANTIQUITY XXXI, 1957

Jericho and its Setting in Near Eastern History

by ROBERT J. BRAIDWOOD

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THE recent general accounts of Miss Kathleen Kenyon's brilliant series of campaigns at Tell es-Sultan, the mound in Jordan generally conceded to be that of ancient Jericho, will be well known to readers of ANTIQUITY (cf. especially Wheeler, no. 119, 132–6; Kenyon, no. 120, 184–95). Sir Mortimer Wheeler and Miss Kenyon interpret the prehistoric archaeological assemblages of the two basal phases of Tell es-Sultan as evidence of *civilization* (Miss Kenyon's italics), and the surprisingly early radiocarbon dates for the upper portion of the second basal phase of the site are accepted at face value. Miss Kenyon, in fact, believes (*Ill. London News*, no. 6,123 [13 October, 1956], 612) that Tell es-Sultan was already a ' town ' at a date ' which must approach the 8th millennium ' B.C.

In consequence, people having begun looking at me—as the excavator of Jarmo—with that look which I suppose is usually reserved for bridegrooms left waiting at the church. My reaction, both in terms of what I know of the comparative archaeology of the Near East and of cultural process in general is—in pure Americanese—that it doesn't figure!

Thus it seems that I should record my first impressions in the matter, brief and incomplete as they are. I can base myself only on the general accounts which have appeared so far. A careful reading of the papers cited above, for example, shows that both Miss Kenyon and Wheeler—for all the sound and fury about ' civilization ' and ' urbanization '—are in reality properly diffident in their interpretation. Miss Kenyon sums up her presentation (p. 193) as ' a pure hypothesis'. But since both the Kenyon and Wheeler interpretations point in one general direction and offer no real alternative, this essay is in part a critical appraisal of their interpretation and in part a brief attempt to suggest that an alternative interpretation is possible. Not having actually handled the Tell es-Sultan materials, it is only proper that my interpretation should be even more diffidently offered than that of Miss Kenyon and Sir Mortimer. I will set forth my reactions under five headings: (1) The generalizations about civilization; (2) the radiocarbon dates; (3) the comparative archaeology; (4) the ' oasis ' theory; and (5) the implications for the general theory of cultural process. I will do my best to side-step that common occupational hazard of prehistorians that makes them believe everything must have started in the exact area where they themselves have been most active. In fact, neither Miss Kenyon nor I have yet formally answered Professor Coon's claims (*The Story of Man*, 1954) for the Caspian foreshore. I shall not attempt to cope with *that* here!

THE GENERALIZATIONS ABOUT CIVILIZATION. Some eight acres of the basal core of Tell es-Sultan appear to have been surrounded with a sequence of masonry ring walls. Reasoning from the domestic architectural concentration within the walls, shown by tests made so far, Miss Kenyon postulates a population of some three thousand people, and Wheeler ' a tolerably developed social organization'. From this the idea of a *civitas*, and therefore ' civilization ', is derived. In reply to Sir Leonard Woolley's worries about the usage ' civilization ', Sir Mortimer has shifted his ground to ' urbanization ' (ANTIQUITY, no. 120, 224–5).

My first reaction to all this was in terms of those highly appreciated lines of the

opening song in the new Broadway musical comedy, *My Fair Lady* (née *Pygmalion*), where Mr Rex Harrison (as Professor Higgins) sings:

> ' There even are places where English completely disappears,
> In America, they haven't used it for years ! '

I simply could not understand what Miss Kenyon and Sir Mortimer were talking about, in terms of the evidence they cited. My second reaction is that most American prehistorians—unlike most of their British colleagues—come up through anthropology and the general philosophy of the social sciences rather than through the classics and the humanistic philosophy. Hence, I reason, Miss Kenyon and Sir Mortimer probably *are* using a different language. On pain of being branded a ' purist ' by Miss Kenyon, I would like to suggest to the editor of ANTIQUITY that he invite one of the great British cultural theorists (e.g. Evans-Pritchard, Firth, or Fortes) to comment on the implications of Miss Kenyon's word formula ' Urban pre-pottery Neolithic Jericho ' (which, incidentally, gives me great personal satisfaction for the private little war I wage with any use of the term ' Neolithic ').

It is not the lack of writing which bothers me in the claim for ' civilization ' or 'urbanization' at Tell es-Sultan. The Inca had no writing (the *quipu* was a simple mnemonic aid at best),but their culture is usually accepted as one of the independent early civilizations. But where are some of the other criteria for civilization at Tell es-Sultan ? Even against Gordon Childe's ten criteria, mainly archaeologically implied (*Town Planning Review*, XXI [1950], 3–17), I suggest that the Tell es-Sultan evidence does very poorly. Size of settlement I would grant only in a very relative sense, and ' monumentality ' for the ponderous ring walls and tower even more relatively. But what traces are there, in the pre-pottery and pre-metallic milieu of Tell es-Sultan, of the full-time technical specialists which Childe calls for ? I certainly take Childe to mean economic differentiation of labour, not the less formal specialization of labour by sex, age, or season, which must be far older than Tell es-Sultan in its beginnings. Evidence for Childe's other criteria of ' civilization ' evades me just as completely.

In what I take to be the generally accepted sense of either ' civilization ' or ' urbanization ', in meaningful culture-historical terms, I myself am certainly not inclined to grant their usage for the Tell es-Sultan evidence as I can see it. To be explicit, I would cite as an example the culture-historical usage given ' civilization ' by Robert Redfield in *The Primitive World and its Transformations* (1953) or in his Huxley lecture (*J.R.A.I.*, LXXXV [1955], 19–32). One naturally yields his colleagues liberties in over-emphasis in their first flood of enthusiasm, since he has doubtless transgressed in this same way himself. It did, however, give me satisfaction to find in Wheeler's answer to Woolley that ' . . . the new discoveries at Jericho extend town-building back beyond the pre-existing limit, and that was the main point of my article . . . ' and that ' " civilization " is too big a word ' for what he was talking about. With this I certainly agree.

THE RADIOCARBON DATES. Two radiocarbon dates now exist for the upper portion of the second basal phase of Tell es-Sultan (GL. 28, 8200 ± 200 = 6250 B.C.; GL. 38, 7800 ± 160 = 5850 B.C.). At first sight, the fact that there is a pair is impressive; so also is the very great care which Professor Zeuner (ANTIQUITY, no. 120, 195–7) has clearly taken in the preparation and counting of the samples, and his observation that ' the dates obtained may have a tendency to be on the young side '.

On second thought, it is clear, as Professor Zeuner well realizes (*11th Ann. Rep.*, Inst. of Arch., Univ. of London [1955], 1–8), that the radioactive method of dating still has many difficulties. Some of these are now being anticipated in the laboratory (e.g. W. S.

Broecker and J. L. Kulp, *American Antiquity*, XXII [1956], 1–11). But other difficulties which concern possible contamination while the sample was still in its archaeological context, or which might appear during its removal from context and its shipment and preparation, are still to be resolved. The possibility of the reduction of radioactivity—in the ' cleaning ' of specimens from an alkaline context—is still an imponderable. So, also, is the Elsasser, Ney, and Winckler (*Nature*, vol. 178, no. 4,544, 1 December, 1956, 1226–7) concern with possible changes through time of the world assay of radiocarbon, due to fluctuations in the geomagnetic field. It seems increasingly clear that a single series of counts from a given context may not be taken straight off, but rather acquire meaning as they become part of a general archaeological pattern for a time sequence of some areal extent (1).

Taking my clue from the general pattern of the dates now available for the area, before the pair from Tell es-Sultan appeared, I have considerably lowered my own chronological sights. The implication for the theory of culture change in this lowering is as follows: given a relatively late appearance of food-production which was truly effective, the rate of cultural acceleration which followed was very rapid. This has its clear technological parallel in what has happened since the Industrial Revolution. The implication of the high dating favoured by Kenyon and Wheeler would seem to be: given a relatively early appearance of effective food-production, the rate of cultural acceleration was at first rapid but then slacked off to a plateau for several thousand years. If I follow Miss Kenyon correctly (p. 192) Tell es-Sultan was gradually depressed from town to village as time passed. She thus suggests that the plateau was a lack-lustre affair that slumped off as time passed. Somehow, I deplore this interpretation. I also deplore the well-known bandwagon phenomenon for generally earlier dates which unquestioning acceptance of GL. 28 and GL. 38 is bound to stimulate (2).

Clearly, the differences between my reactions to the Tell es-Sultan radiocarbon dates and those of Kenyon and Wheeler will not be resolved quickly. In 1954–5, Professor F. R. Matson, National Science Foundation grantee to the Iraq-Jarmo field staff, collected some fifty-two radiocarbon samples covering the late cave and early village range in western Asia. When Matson has completed the time-consuming process of ' cleaning ' these samples, and their counting is done, a comprehensive time grid for the later prehistory of western Asia should become available. Unfortunately, it was impossible for us to secure a Natufian sample. A radioactive carbon date for the Natufian is of great importance, both in the present issue and generally.

Although they explicitly deny that the point is binding, Kenyon and Wheeler (e.g. *Antiq. Jour.*, XXXVI [1956], 167) both imply that the 45 ft. of accumulation of debris took ' a long time ' (Kenyon, *Ill. London News*, no. 6,123 [13 October, 1956], 611). From information we could gather in a variety of villages in the piedmont of Iraqi Kurdistan, the average life expectancy of the *touf* or *libn* house of an ordinary *fellah* is about fifteen years. The debris product in mud is about 2 ft. in height. At this rate, 350 years could easily accumulate 45 ft.

Until a general pattern of radiocarbon dates is firmly established by means of many more runs, I myself am decidedly inclined to caution in the acceptance of the pair of dates from basal Tell es-Sultan, and certainly in any implication for a beginning of town life in the 8th millennium B.C.

THE COMPARATIVE ARCHAEOLOGY. This section is necessarily restricted—as regards Tell es-Sultan—since only the materials mentioned in the general accounts are so far available. From what I understand of the internal evidence and its comparative bearing on the rest of Near Eastern archaeology, I see no reason to change the position which my wife and I took several years ago (*Journ. World History*, I [1953], 278–310).

Quite understandably, Miss Kenyon gives us few details of the newly discovered and incompletely analysed ' hog-back brick ' phase. We are told that the earlier levels of the ring wall and the tower pertain to this earlier phase and that its houses were circular. More information is available concerning the succeeding ' plastered floor ' phase. Taken together with the older descriptions of the late Professor Garstang, we begin to form a general picture of the ' plastered floor ' phase assemblage. This assemblage is remarkable for its masonry ring wall and its bizarre skull cult, also—perhaps—in its lack of pottery vessels. Otherwise, the general items in its catalogue sound in no way extraordinary (cf. Robert J. and Linda Braidwood, *op. cit.*), although they do suggest a local cultural facies distinct from other local specializations in the Fayum, Syro-Cilicia, north-eastern Iraq, and both northern and south-western Iran.

The plastering and burnishing of house floors and walls is available in basal Byblos, Hama, and Ras Shamra, and thumb-indented bricks occur at Teleilat el-Ghassul, only some ten miles from Tell es-Sultan (normally, mud bricks begin occurring at about the Ubaidian horizon). The eastern sites (cf. Iraq, Iran) of the same general typological complexity also yield clay figurines of human females and animals. The Tahunian flint industry is in no way remarkable against the same general background. Nor was it entirely clear to Mrs Crowfoot-Payne that her ' Tahunian II ' was the same ' Tahunian ' as that from the Judaean caves. Mrs Crowfoot-Payne (*L.A.A.*, XXII [1935], 183; XXIV [1937], 50) accounts for obsidian in both her ' Tahunian II ' and her underlying ' microlithic industry ' of Tell es-Sultan. The items in ground stone sound quite usual: the relative scarcity of celts may simply reflect a non-forested environment such as that of today. We would insist (*op. cit.*, 296) that basal Tell es-Sultan cannot be generally discussed without reference to the comparable materials of the Judaean hill site of Abou Gosh, where, incidentally, celts are more common.

Since Lewis H. Morgan's suggestion (*Ancient Society*, 1877) that pottery was *the* marker for the appearance of his ' lower status of barbarism ' (usually equated with ' Neolithic ', although Morgan introduced domestication only with his ' middle status of barbarism ') archaeologists have tended to set great store by the appearance of this one trait. But along with Tell es-Sultan there is also most of Jarmo, Khirokitia, and the basal level of Kili Ghul Mohammed in the Old World, and basal Huaca Prieta in Peru, which exhibit mound sites with well-expressed domestic architecture, but no pottery. At Huaca Prieta the preservation factor is high, and it is clear that gourds and basketry were used for receptacles. On the other hand, both the archaeological and ethnological literature contains instances of pottery-using food-collectors. Clearly, the appearance or non-appearance of pottery need no longer be assumed as a mark of major importance in a culture-historical sense. The negative formula ' pre-pottery Neolithic ' grows increasingly meaningless.

Miss Kenyon's sense of the comparative archaeological statigraphy of Palestine appears to remind her that, in accepting the very early dates for basal Tell es-Sultan, she must necessarily create a long gap in the generally accepted sequence. She attempts to adjust this by equating Jericho VIII with the Yarmukian (Sha'ar ha-Golan), and both of these with Byblos A, and hence with basal Ras Shamra, Mersin, and Hassuna. We ourselves (*op. cit.*, 297) were unable to decide whether the Yarmukian itself should be equated to Byblos A or B—we can still see points in either equation, although we favour the latter. But Byblos B is a sort of Uruk (north Iraqi Gawran) offshoot at best, and this would check rather well with the very good equation between Jericho VIII and Amouq phase D (cf. Ernest Wright, *B.A.S.O.R.*, no. 122 [1951], 52–55), in which there are far stronger elements of comparison than the rather ubiquitous incised herringbone decoration of pottery.

Amouq phase D is a peripheral Halaf-Ubaid transitional range in Syro-Cilicia. This also fits nicely with the accepted (e.g. W. F. Albright, *The Archaeology of Palestine*) immediately pre-Ghassulian positioning of Jericho VIII (3). Miss Kenyon must simply choose between a long gap in the record, or her very lacklustre cultural plateau, which leaves practically no trace in the archaeological record. Jericho VIII cannot be as early as Byblos A or basal Mersin or Hassuna, or anywhere near them. A generalized location of Jericho VIII with Amouq D, and of the Yarmukian with Byblos B, with the latter pair overlapping or running slightly later than the former, is quite feasible.

THE OASIS THEORY. I shall not speak to the point of an ' Oasis Theory of Civilization ' or an ' Oasis Theory of Urbanization ', Wheeler's alternate formulae, because I do not think either ' civilization ' or ' urbanization ' are at issue. If, however, as Wheeler says, the ' main point ' is a *relatively* early establishment of towns (and I insert and insist on the word ' relatively '), I would agree that oases may have been one place where this could have happened. River-lands (*sic* Eridu) and coastal sites (*sic* Byblos A) might be other examples, although the relatively unplumbed basal levels of, for example, Tepe Gawra, in the hilly-flanks zone, also come to my mind. Such towns, perhaps already incipient religious, political, and market centres for a district, must have had ancillary villages about them in somewhat less-favoured spots, and I suggest that the Abou Gosh hill village may have borne this relationship to Tell es-Sultan, although rather at the end of Miss Kenyon's second basal phase as we now understand the materials from the incomplete Abou Gosh exposure.

But if the oasis theory is to pertain to the appearance of an effective village-farming community way of life—and there is much in what Kenyon and Wheeler (e.g. *Antiq. Jour.*, XXXVI [1956], 166) write which suggests that *this* is what they mean, although they talk about ' civilization ' and ' urbanization '—then I maintain that the evidence is increasingly against it.

In terms of the history of ideas, the oasis theory seems to go back at least to the earlier writings of C. E. P. Brooks, in which it was postulated that a post-glacial readjustment of the Atlantic storm track would have brought about desiccation in the North African and west Asiatic regions. The cultural implications drawn from Brooks' postulate—repeated so often by archaeologists and general historians (e.g. Toynbee) that they have acquired the apparent character of truisms—are that men, plants, and animals retreated to oases *and* river-lands with increasing desiccation, and that domestication followed propinquity. As archaeological knowledge has increased the river-lands factor has dropped out, but we apparently now have a revival of the oasis hypothesis by Wheeler and Kenyon (and, incidentally, by Woolley ' . . . an oasis origin for agriculture is virtually certain ').

I am not at home in the meteorological or climatological literature, but I can find no meteorological colleague in the flourishing department at Chicago who will admit that the climatic renovation was as simple as Brooks first suggested. Hans Bobek's (*Geogr. Jahresbericht Österreich*, XXV [1954], 1–42) recent survey of the evidence leads him to suspect that a uniform ' pluvial girdle ' of the sub-tropical latitudes did not exist in the Pleistocene. If we focus on Asia west of the Iranian *kavirs*—that is, where the beginnings of effective agriculture based on the wheat, barley, sheep, goat, pig, and cattle complex undoubtedly had its start—we are very hard put to find clear and significant traces of desiccation within the pertinent time range. For example, Haas (*Israel Explor. Journ.*, II [1952], 44–6) presents impressive evidence that ' . . . the biotype of layer B of Abu Usba was not essentially different from that found on the spot today, except for a somewhat denser vegetation of the forest, correlated with a somewhat larger quantity of rain.' So far, our Jarmo evidence has exactly paralleled what Haas describes, save that the vegetation

cover in the Chemchemal valley was evidently a parkland of scattered oak; we reckon subsequent deforestation to have been due to overgrazing.

The field research necessary for a meaningful paleoclimatological, paleobotanical, and paleozoological history of post-glacial south-western Asia is only beginning. We have a long way to go. But for some years I have taken my own clue from hints such as those suggested above, which point to no great or significant environmental difference in the time range of *c.* 10000 to 5000 B.C. from that of today in south-western Asia. Hence I have focused my own attention on the hill-flanks and intermontane valleys of the ' Crescent ' and away from the more arid environs of the riverlands and oases. I feel justified in letting my thinking proceed from present-day rainfall and vegetation maps. I am not very interested in Syria behind the lee of the Anti-Lebanon (which seems to attract Miss Kenyon and Sir Mortimer), as it has little rainfall, and I would not lay much of a bet on the lush Syro-Palestinian littoral: I've a hunch it was *too* lush. I have my eye particularly on the flanks of the Amanus and in north-west Syria, on the Anatolian plateau flanks which lie about the great bend of the Euphrates, and on the western flanks of the Zagros with a possible fingering up towards Lake Urmia. I am, as yet, non-committal about the rain-watered Jordanian uplands.

I will suggest two points in my thinking about the oasis environs of Tell es-Sultan and agricultural beginnings. Professor Charles A. Reed, the National Science Foundation's zoological grantee to the 1954–5 Iraq-Jarmo field staff, tells me it is a biological incongruity to expect that the tendency of the animals pertinent to domestication would have been to move *downwards* to oases or river-lands *if* desiccation had set in: any such retreat would rather have been *upwards* into the rain-watered hill country. Although there is an older observation for it at somewhat lower altitudes (cf. (4)), Hans Helbaek (personal correspondence, but cf. *9th Ann. Rep.*, Inst. of Arch., Univ. of London [1953], 47) notes that wild wheat is usually referred to as a medium altitude plant; in north-eastern Iraq, his record of *T. dicoccoides* is for *c.* 750–1,000 metres above sea-level. George E. Post (*Flora of Syria, Palestine, and Sinai*, 1933) speaks of *T. dicoccoides*' occurrence in ' rocky places in the mountains ', and of the occurrence of *T. monococcum* in ' grassy places in the mountains '. Helbaek (unpublished MS.) is of the opinion that wild barley, although more widespread than wild wheat, came into cultivation as a weed in early wheat fields.

The elevation of Tell es-Sultan is 233 metres *below* sea-level.

It is not, however, my understanding that clinching arguments can yet be offered for either an ' oasis ' or a ' hilly-flanks ' theory for the origins of the effective village-farming community way of life in western Asia. A great deal more must yet be learned by archaeologists and natural scientists working in the field together and in easy communication. Such a study will naturally have to include the oasis of Jericho, with its spring and—I suspect—somewhat precious below sea-level ecological situation. I am not yet ready to grant that this very preciousness was the determining factor in the achievement of a new way of life.

THE IMPLICATIONS FOR THE GENERAL THEORY OF CULTURAL PROCESS. I am very conscious that the conclusions I draw from an inspection of the radioactive carbon dates listed above show my commitment to a point of view. This is that the late prehistoric Near East was the scene of a single generalized area co-tradition, which had a more or less uniform development—with specific points of acceleration—through time, in the interconnected regions where its food-getting economy was environmentally practicable. The conception of the area co-tradition by the Americanists (e.g. Wendell C. Bennett, *American Antiquity*, Memoir 4 [1948], 1–7) somewhat resembles Frankfort's (*Studies Presented to F. Ll. Griffith*, 1932, 445–53) earlier thinking about a basic Hamitic substratum in north-eastern Africa. The

area co-tradition implies, in fact, that kind of basic cultural community (better: commonalty) which, for example, the present bearers of the specific cultures of western Europe—whether Scandinavian, German, French, English, etc.—all hold in common.

The late prehistoric area co-tradition of the Near East is archaeologically manifested to us by the traces of the same food-plants and food-animals, the same generalized architectural habits, the same generalized way of making pottery (Near Eastern pots look quite different regionally, but have an over-all general similarity if compared, for example, to Mississippi valley pots), and of flint tools, ground stone tools, and so on. Some items, such as clay figurines of the ' mother-goddess ' type, and obsidian bladelets, are probably more specific manifestations. The case of obsidian, restricted in its natural occurrences, gives us hints of that loose web of trading relationships maintained between the regionally developing village cultures of the time. I maintain that the general commonalty of artifactual and non-artifactual materials available from the late prehistoric Near East suggests the existence of an area co-tradition, and even implies a sort of *oikoumenê* for the peoples of the area.

I very definitely *do not* think of the traits of commonalty of the area co-tradition as all having had their start on one favoured site or even in one favoured mountain valley. The first conscious planting and selection of wheat may have taken place in Syro-Cilicia; the first goats may have been bred near Malatya; while the first portable pot may have been made near Diyarbakir, and the first *touf* wall built in Iraqi Kurdistan. We will probably never know. What I am prepared to believe is that there was intercommunication within the *oikoumenê* of the area co-tradition, and that word of new ideas travelled fast. I suggest that Frankfort's idea of a crystallization, in times of such cultural crises and change, is apt.

We do not yet know much of the mechanics or causes of this apparent crystallization of the effective village-farming way of life, which lies at the beginning of the area co-tradition of which we speak here. I have speculated (e.g. ANTIQUITY, no. 120, 223–4) that an as yet only vaguely conceptualized era of incipient agriculture lay before it. This *may* have had its own even vaguer area co-tradition, and this *may* even be what Sauer is talking about in the theory from which Wheeler quotes (4), but this part of the idea is most tenuous. In our opinion (Robert J. and Linda Braidwood, *op cit.*) we so far have traces of this era only in archaeological materials of the Natufian and Karim Shahir type, and both are peculiarly difficult to interpret. If I should be pushed into giving a guess-date for the crystallization of the era of the village-farming community from the era of incipient agriculture which lay before it, it would be about 5000 B.C. $\pm$ 500 years.

Against this theory of an area co-tradition with its *oikoumenê* and sense of cultural process, Miss Kenyon seems to place herself in a multiple origins position. On page 192 of her ANTIQUITY article, we find her postulating ' a large number of little beginnings of civilization ', some of which seem to fizzle out and get ' lost '. She tells us that the basal Tell es-Sultan materials must exhibit such a case, although we noted nothing extraordinary or exotic about the objects in the usual small-object categories of her assemblage. From the area co-tradition–*oikoumenê*–cultural process point of view, I suggest that Miss Kenyon and Sir Mortimer are asking us to believe an impossibility. They show us a *town* of eight acres in area, surrounded by a rough stone ring wall with such implications of societal complexity as this may imply, but with a quite normal general assemblage for its milieu. They ask us to believe that this town was already established at a date ' which must approach the 8th millennium ' B.C. Unless the whole fabric of other dates and reasoning is wrong, the co-tradition itself cannot have been so early. I, for one, do not believe that Tell es-Sultan can have preceded its own co-tradition.

* * *

In conclusion, I wish to recall the diffidence with which I offer my own interpretation. I cannot accept the Kenyon-Wheeler generalizations about ' civilization ' as pertinent to their evidence. I suggest that the basal Tell es-Sultan evidence implies an incipient town in a somewhat marginal region. I urge extreme caution in the acceptance of the pair of radiocarbon dates until a much more adequate time-grid is available for the area. I suggest that when Miss Kenyon has had time to examine the comparative archaeological position of her basal material, in relation to other early village and town site materials now available, she may well present different conclusions. I maintain that the trend of evidence acquired by the natural scientists is away from the oasis theory, and towards the hilly-flanks of the ' Crescent ' as the locale for the crystallization of the early village-farming community way of life. Finally, I suggest that Miss Kenyon's and Sir Mortimer's interpretation runs strangely counter to the general theory of cultural process, nor can I see that their counter-theoretical case has any particular strength.

I heartily join Sir Mortimer in my anticipation of the results ' when Miss Kenyon has served another term of five years' hard labour in the Jordan Valley ' and sincerely hope that political circumstances will not prevent the early resumption of work at Tell es-Sultan.

Notes

(1) Examples may be cited of the recounting of radiocarbon dates which have given both earlier (the Adena-Hopewell series) and later (the Khirbet el-Bitar ' Ghassulian ') datings on the recount. These were cases in which the order of error was far greater than could be accounted for by the somewhat greater accuracy of gas-chamber counting as against the older dry-carbon method.

If the Caspian, Afghanistan, Sudanese, and Libyan dates are ignored as probably not pertinent to the problem, the presently available radiocarbon dates from the Near East run as follows:

C813, Negada hair, S.D. 58–67	2766 ± 310 B.C.
C267, Hemaka beam, 1st Dynasty	2933 ± 200 B.C.
C812, Negada hair, ' Negada II '	3066 ± 290 B.C.
GL24, Jericho, ' late chalcolithic '	3260 ± 110 B.C.
C463, El Omari, mid. predynastic	3306 ± 230 B.C.
W245, Khirbet el-Bitar, ' Ghassulian '	3325 ± 150 B.C.
C819, Byblos, ' prim. inst. urbaine '	3364 ± 300 B.C.
C817, Gawra 17–18, middle Ubaid	3446 ± 325 B.C.
C814, Negada skin, S.D. 34–38	3624 ± 300 B.C.
C811, Negada hair, S.D. 36–46	3665 ± 280 B.C.
C810, Negada hair, S.D. 34–38	3791 ± 300 B.C.
C457, Fayum A grain, K pits	4145 ± 250 B.C.
C550, C551 } Fayum A grain, K pits	4437 ± 180 B.C.
C742, Jarmo charcoal	4654 ± 330 B.C.
C743, Jarmo charcoal	4743 ± 360 B.C.
C113, Jarmo shell	4757 ± 320 B.C.
GL38, Jericho, charcoal (2nd pre-pot. phase)	5850 ± 160 B.C.
GL28, Jericho, charcoal (2nd pre-pot. phase)	6250 ± 200 B.C.
W179, Shanidar B charcoal (Zarzian)	10000 ± 400 B.C.
W178, Shanidar C charcoal (Baradostian)	27500 ± 1500 B.C.
C818, Hazer Merd ash (Mousterian)	older than 25000 B.C.
W180, Shanidar C charcoal (Baradostian)	older than 32000 B.C.

Unfortunately, there is no way to graph the dates in the above list; if time were to be used as an ordinate, then increasing cultural complexity would be the natural abscissa, but we could not quantify it objectively. Nor do we know the nature of the curve of acceleration of cultural complexity, which is, of course, a great part of the point at issue. One can, so far, only make his judgment by an inspection of the dates in the list, bearing in mind what these dates imply as to archaeological assemblages.

(2) For example, it depressed me to note that Max Mallowan, *Twenty-Five Years of Mesopotamian Discovery* (1956), 11, now suggests '*c.* 5000 B.C. rather than *c.* 4000 B.C.' for Arpachiyah.

(3) After the above was written, Professor Wright reminded me of the important bearing which the site of Ghrubba will have on the understanding of basal Tell es-Sultan. The low mound of Ghrubba lies near the Allenby Bridge, some eight miles from Tell es-Sultan. Wright has provided me with galley-proof of Mr James Melaart's short report (Dept. of Antiq. Jordan, *Annual*, III) on a sounding at Ghrubba, and also with the MS. report of his own 1956–7 field and museum reconnaissance in Palestine (forthcoming *Year Book of the American Philosophical Society*). In February 1955, Mr Lankester Harding, then Director of Antiquities in Amman, showed me the Jordanian Museum's selection of the Ghrubba material, and I am interested to note that Melaart accepts the possible equation with Amouq B, which I then suggested to Harding. On this basis, Melaart places the flourish of Ghrubba before 'Jericho IX' (the upper pottery bearing aspect of Miss Kenyon's second phase, in the Garstang terminology). Wright himself (MS. cited above) would place the Ghrubba material after Jericho IX, but before Jericho VIII, and an examination of the whole yield of the Ghrubba sounding illustrated by the Melaart galley-proofs convinces me that Wright's position is the correct one. Nor am I so sure of some other of the equations suggested by Melaart.

From Wright's MS. we also learn that the Yarmukian 'is definitely in the same horizon as Jericho VIII', that there is stratigraphic evidence of a Yarmukian-Jericho VIII type of material succeeded by the Ghassulian, and that M. Stekelis has exposed an open site with 'Tahunian flints' together with various installations of which some were 'probably huts'.

Clearly much more knowledge of the comparative archaeological stratigraphy of Palestine (as well as of the rest of western Asia) is becoming available, and the interpretation of the Tell es-Sultan materials cannot be an affair of 'splendid isolation'.

(4) Wheeler seems impressed with Sauer's generalizing. The distinguished Harvard botanist Paul Mangelsdorf remarked (*American Antiquity*, XIX [1953], 90), 'Indeed, if one sought, as an exercise in imagination, to design a completely untestable theory of agricultural origins and dispersals, it would be difficult to improve upon this one.' To my mind, this must also summarize the prehistoric archaeologist's reaction to Sauer's theory.

In his presidential 'Anniversary Address' (*Antiq. Journ.*, XXXVI [1956], 166) Wheeler also cites Sauer's theory that '. . . the sickle is an argument for already domesticated grain', based on the supposition that the *rachis* of all wild wheat is brittle, and would scatter, making use of the sickle a disadvantage. This is a very complicated matter and the last word from the botanists is not yet in. We trace the observation to Aaron Aaronsohn (U.S. Dept. of Agric., Bureau of Plant Industry, *Bulletin* no. 180, 1910), but have not confirmed it in the more modern botanical literature. It is Aaronsohn, incidentally, who remarks on wild wheats at lower elevations in Palestine. There is a further answer to Sauer's point, however. The earlier flint sickle blades *may* have had nothing at all to do with the cereal grains. Dr Ali Rawi and Dr Hansford, of the Iraqi Directorate-General of Agriculture, informed us that ripening reeds would have the same silica concentration in the plant skeleton as does ripening cereal grain. Sickle-sheen on flint blades may only be due to the reaping of reeds, the use of which is attested at Tell es-Sultan, Jarmo, and many other early sites.

Reply to Professor Braidwood

by KATHLEEN M. KENYON

PROFESSOR BRAIDWOOD has put up a noble defence of his understanding (I use his phrase) of the crystallization of the early village-farming community way of life on the hilly flanks of the Fertile Crescent. He disarmingly starts by saying that he will do his best ' to side-step that common occupational hazard of prehistorians that makes them believe everything must have started in the exact area where they themselves have been most active '. The fact of course is that neither he nor I do so; we are both prejudiced in favour of our own pet theory and pet site. But the more we are so, and the more we endeavour to ferret out the facts to support our views, provided we are candid in the presentation of the facts we discover and in the acceptance of the proved facts (but not theories) of the other school, the more rapidly will knowledge advance. I will trust Professor Braidwood to be thus candid and I hope he will likewise trust me. But I would like to remind him that he *was* suffering from jaundice when he visited Jericho, resolutely rising from his bed of sickness so as not to miss his party's visit to the site, *and* that at that stage we had not made our most exciting discoveries.

I cannot expect the Editor of ANTIQUITY to bear with a lengthy reply to Professor Braidwood's interpretation, for he has already been more than generous in the space he has given to Jericho. But I feel that there are a few points which I might usefully make as a contribution towards further work in the field which we are all agreed is necessary. The point made by Professor Braidwood with which I would most strongly agree is that ' field research . . . is only beginning ' in the various environmental problems. From Jericho itself we still await the specialist's examination of most of our specimens, and that is only one site. Until we have these facts our theories must remain theories.

The first point I would make is on the question of absolute date. In a quarter-century which has added Halaf, then Hassuna, and then Jarmo to the accepted sequence, it would ill-become us to say that further additions of greater antiquity are impossible. But I would entirely agree with Professor Braidwood as to the care with which we should use radio-carbon dates. Indeed, Professor Zeuner, who is in charge of the examination of our specimens, has emphasized perhaps more clearly than anyone else the possible sources of error. Two results are certainly not enough (though it is perhaps not unfair to point out that Professor Braidwood is satisfied with three for Jarmo, and was not disposed to be unbelieving when there was only one, even though it suggested a hitherto unsuspected antiquity). Work is proceeding on other specimens; the results are not yet ready for publication, but at least it can be said that they do not conflict with those already obtained.

As to Professor Braidwood's suggestion that any sort of yardstick of a rate of accumulation of deposit on the *tell* of two feet every fifteen years can be employed—to use Professor Braidwood's words, it just doesn't figure. The history of rise, collapse, denudation and embankment is far too complex. To take only an example from Jericho: For the period 3000 B.C. to 1400 B.C. we have reasonably accurate criteria; by the yardstick suggested, that should be represented by 106 feet. In the forty-five feet of deposit to which he refers, this period is actually represented by six feet, and in the best preserved part of the *tell* by only thirty-six feet.

The second point is with reference to his observations on my reading of the com-

parative archaeology. Certainly individual features in the ' plastered floor ' phase can be found elsewhere. But the floors of Byblos are associated with flimsy huts, utterly unlike the substantial houses of Jericho. The thumb-indented bricks of Ghassul are utterly unlike those used in these houses. That features which can be given the same names occur in other sites of Western Asia is no evidence of an ' area co-tradition '.

I agree entirely that pottery is not a necessary type-fossil of a Neolithic assemblage, and I have in fact repeatedly said that the absence of pottery is not in itself evidence of antiquity. But the use of the term pre-pottery Neolithic at Jericho is justified by the fact that there are two succeeding Neolithic stages with pottery from which it must be distinguished.

Some of Professor Braidwood's difficulty in accepting my reading of the comparative archaeology may be lessened if I clear up some misconceptions about the Jericho evidence under which he is obviously labouring. This is most clearly shown in his footnote 4. Jericho IX is *not* the upper pottery-bearing aspect of my second phase. The original Jericho IX was stratigraphically completely mixed. There is a *complete* break between the pre-pottery phase and that of the first Neolithic pottery. Between the two I am perfectly prepared to admit a gap, perhaps a big one. This same misconception occurs earlier, in the statement that ' Tell es-Sultan was gradually depressed from town to village as time passed '. As far as evidence goes, there is no ' depression ' during the pre-pottery phase; for reasons as yet, and perhaps for ever, unknown, it comes to an end. Its successor, perhaps after a gap, is retrogressive. With strong self-control I forbear to comment on ' lack-luster '.

Moreover, what was published as Jericho VIII was also stratigraphically completely mixed, and therefore any attempts to compare it as an entity on the published evidence is not valid. But certain elements in it, notably the pottery, which I prefer to call Pottery Neolithic B, do quite definitely correspond with Yarmuk, and when M. Dunand recently saw it, he agreed emphatically with its correspondence with his Byblos A. The correspondence of that with Yarmuk, moreover, has recently been confirmed by the discovery at Byblos of the curious pebble-figurines so characteristic of the former site. I therefore reply to Professor Braidwood's categorical statement that ' Jericho VIII cannot be as early as Byblos A or basal Mersin or Hassuna, or anywhere near them ' with a blunt contradiction, modified only to the extent that Professor Braidwood has not yet got the true evidence available to him owing to the unreliability of Jericho VIII. At any rate, Jericho Pottery Neolithic B *is* as early as Byblos A. Thereafter there was probably a gap, within which falls the Ghassulian.

If, after further tests now in progress, the radiocarbon datings are sufficiently confirmed to secure general acceptance, I shall therefore not have Professor Braidwood's difficulty in accommodating the evidence of comparative archaeology to them. Before Pottery Neolithic B there is Pottery Neolithic A, with the burnished red-on-cream decoration as its outstanding characteristic. Before Pottery Neolithic A is the pre-pottery ' plastered floor ' phase (with a long succession of building stages after those for which the radiocarbon datings have been obtained). Between each of these there is a break on the evidence of stratification and of culture, and how long a gap there may have been there is no means of knowing. After the Pottery Neolithic B there is another gap, the archaeological evidence for which is reinforced by Professor Zeuner's identification of a fossil soil, for the formation of which he estimates a period of at least three hundred years. Then we come to the radiocarbon date of 3260 B.C., which Professor Braidwood is apparently prepared to accept because it does fit in with his ideas.

I do not propose to discuss the use of the terms urbanization or civilization, nor the

Oasis Theory, for this would tax the Editor's patience too highly. Partly it is a matter of terminology, partly of an anthropological as against an archaeological approach. I simply maintain that I cannot personally class Jericho with the early villages of which we have evidence elsewhere. Even Professor Braidwood goes as far as to call it an ' incipient town .'

The main disagreement between us lies in the fact that Professor Braidwood's thesis is that Jericho must fit into the chronological and developmental framework of the early villages as at present known. Mine is that it lies outside it. But even if future evidence proves that I have overstated the case, and early Jericho is lowered to a mere 5000 B.C., contemporary with Jarmo, and even if it may only be called an incipient town, an incipient town of 5000 B.C. would still be a remarkable development.

ANTIQUITY XXXI, 1957

The Brummagem Philhellene

by A. J. B. Wace

YOU will not find the name of the Philhellene George Fanshawe Chubb in the *Dictionary of National Biography*, although that does include a purely mythical Philhellene Kirkman Finlay. Though he lost his life in the cause of Greece, Chubb is never mentioned by Finlay, Trikoupes, Gordon, or by any other historian of the Greek Revolution. He appears, like Melchizedek, to have had no ancestors and no posterity, and whether he was really a native of Birmingham or not we cannot tell. We know nothing about his family or his education or his profession, or indeed anything at all about him till he became a Philhellene.

Chubb seems to have been personally unpopular. It was Byron who in a letter to Murray called him the Brummagem Philhellene. He makes rude remarks about his personal appearance, the untidiness of his dress, his red hair, his sensual lips, and his silly screech of a laugh. Chubb too was fidgety and nervous, and was frightened of thunderstorms. This was apparently the reason why Hastings ' posted ' him. Among the Finlay papers in the British School at Athens there is a sheet of paper yellow with age and bearing at the four corners the marks of the wafers with which it was fastened to a wall or notice board. On it can still be read quite clearly:

> 'This is to inform all concerned that, since Mr George Fanshawe Chubb has refused me the satisfaction which I demanded from him, I hereby proclaim him a liar and a coward.'

Hastings' signature and the date follow.

The first appearance of Chubb was in connection with the Greek Committee in London, which was busy with arranging help, financial and other, for the Greek cause. Whether Chubb was actually a member or not is not clear. At any rate early in 1824 he was sent to Greece by the Committee with a large sum for the Greek Government. Some say that he carried as much as eighty thousand pounds.

On his way he visited Byron at Mesolongi, whence he made for Nauplia. He reached it safely and handed over the funds to the Greek authorities to whom he was accredited. Then at their request he left for the southern Peloponnese, for the Maina, to seek out that stout old warrior Petrobey and offer him funds to pay his men and purchase arms, ammunition, grain, and other supplies. Petrobey accepted a sum for buying supplies for his people, but refused to take anything for the pay of his men. He said that Greeks, especially his men who were descendants of the ancient Spartans, did not need to be paid to fight for their country. He believed it was different with the English, who were a nation of shopkeepers. He had heard, he said, that in the recent trouble the King of England had had with his rebellious subjects in North America he was compelled to hire Germans to fight for him, because his own people refused, saying the pay was insufficient.

After consulting the Greek authorities at Nauplia and taking the advice of Petrobey, Chubb, who spent some time enjoying the hospitality of the Mainote clansmen there in the southern Morea, decided to proceed northwards into Arcadia to get into touch with the Klephtic chieftains in that district. Kolokotrones provided him with letters and safe conducts and Petrobey gave him an escort of two veterans. The plan was that Chubb should proceed northwards along the eastern side of the backbone of Mount Taygetus,

keeping as far as possible among the foothills of the main range. He thus passed west of Gytheion and avoided also centres like Skoutari and Panitsa. The result was that after three or four days' journey and having risked a quick dash down to the wayside inn of Tarapsa at night to get supplies he found himself in the wild hills west of Gorani. Here, before venturing further, he made for the small ruined Byzantine fort known as Kalaskope. Of this the walls were breached and shattered and only the main keep-like tower was still standing. Here Petrobey maintained a post of some half-dozen men as a look out, as a depot for arms and ammunition and as a station to keep open his communications with the Arcadian chieftains. The fort stood on a lofty and lonely peak jutting out from one of the ridges that run eastwards at right angles from the rocky sierra of Taygetus.

When Chubb made his toilsome way up the steep, stony slope towards this outpost, the evening was wild, windy and stormy. Squalls of rain swept over the bare ridges, driving all before them, and thunder rumbled among the glens of the mountain, whose pine-clad flanks were illuminated now and again by vivid lightning. The party of three, wet and weary, reached the fort, which they welcomed as a veritable haven of refuge, just before nightfall. They found that two of the garrison were away, but the others gave them a warm welcome and such hospitality as they could provide. After Chubb and his companions had eaten a meal of bread and cheese and olives, washed down with strong resined wine, two of the guard took advantage of a lull in the gale to go out to see if they could track and shoot a roedeer. They had seen a small herd of roedeer on the edges of the pine forest a few days before and thought they might perhaps secure one so as to provide their distinguished guest with venison. It is from their report that we know these details.

They did not secure the venison, for later, about midnight, the gale blew again with renewed force and a most violent thunderstorm burst upon them so that they had to lie up in a cave till daylight. Then, when the rain had ceased, they made their way back to Kalaskope. When they reached it they found the main tower also in ruins and could find no trace of Chubb, his two guards, or their two comrades. Even Chubb's mule, which had been stabled in the lower part of the tower, was not to be seen. They could not search the ruins as they had no means to do so, and the great pile of wreckage from the masonry of the tower made it impossible to penetrate even into the basement of the building and effectually blocked all approach. The violent rain too had obliterated all tracks of men and animals.

These two men were closely questioned, because Philhellene circles doubted their honesty and put about the rumour that they had made away with Chubb for the sake of the gold he carried and had then blown up the tower over his body to conceal their crime. It was even hinted that their story was a complete falsehood and that Petrobey himself had burked Chubb. Petrobey and the Greeks maintained that the story was true and that probably Chubb had been overwhelmed in the wreck of the tower blown up, like the Propylaea in Athens, by a flash of lightning or else that terrified by the storm he had wandered out in the darkness and rain and fallen to his death over some cliff. Some even said that Chubb himself was a thief, had bolted with the gold after killing his escort, had joined the Turks, and had become a renegade. Others afterwards went further still, and said he had been seen years later enjoying life in a house and flourishing garden on the romantic shores of the Bosphorus, surrounded by slaves and wives and children. Whatever the truth, the fact remained that Chubb and the rest of his gold had vanished with his escort and his mule.

There the matter still stood when about ninety years later my friend George Evesham was travelling in Laconia and exploring the ancient sites, classical, Byzantine, Frankish and Venetian. He heard the tale of Chubb's disappearance and determined to identify, if

possible, the fort of Kalaskope. He conjectured that it was identical with the castle of Belvedere, which Villehardouin had built in the hills south of the plain of Sparta to protect it from the raiding mountaineers. After some days of rough walking in the neighbourhood of Gorani he discovered what he believed to be its ruins. Indeed, the position on an isolated peak perched on a ridge, and other details, agreed well with the traditional description. Believing that the clue to Chubb's disappearance must lie hidden in the mass of ruin which was all that could be seen of the fort save for a short stretch of broken wall, he applied for permission to excavate. This was readily granted and in 1910 in the early summer, when camping among the foothills of Taygetus was more like a holiday than work, Evesham began operations.

The process of clearing the ruins was long and difficult and he and his men found their progress through the masses of fallen masonry rough and tiresome. Evesham concentrated his attention at first on the highest mass of wreckage which seemed to correspond with the ruins of the tower mentioned in the accounts of Petrobey's men. After more than a fortnight's hard work he was able to clear the base of the tower. Part of the first floor, approached by an outside stone staircase, was still standing though much ruined, and below it was a vaulted basement. One side of the first floor was especially badly shattered and he conjectured that on this side there had been a store of gunpowder which had been struck by lightning and exploded. Everything seemed to indicate that the disaster had been caused by an explosion followed by a fire. Near a fireplace in one corner of the first floor he found four huddled skeletons and some rusty and corroded fragments of metal which seemed to be those of yataghans, pistols, and muskets. Near by was a mass of lead. These were, he said, some of the arms and munitions of the garrison, and the four skeletons were those of Chubb's escort and Petrobey's two men. But where was Chubb himself?

Evesham then began to examine the basement, which was choked with debris of all kinds owing to the collapse of part of the vaulting over it. The door was beneath the steps that led to the first floor. It was a low, dank apartment with no windows or any outlets for ventilation. In one corner stood nine large earthenware jars. In three of these many olive kernels were found. Others contained carbonized wheat, and the remaining two, which were rather stained on the inside and still seemed to have a breath of turpentine about them, Evesham concluded had contained resined wine. In another corner were the bones of a large quadruped which Evesham said must be the mule, and by it were fragments of rotten wood and rusty iron, presumably its pack saddle.

Finally, in the corner farthest from the door, Evesham and his men came upon another human skeleton. It lay crouched in the corner upon a slight rise in the earthen floor of the basement. Evesham cleared all round it with the greatest care and in doing so found a celt, a stone axe of the Neolithic Age. Such stone axes are always called by the Greeks ' thunderbolts ', and so his workmen, who knew the tale of Chubb's disappearance, hailed this as a definite proof that the tower had indeed been destroyed by lightning. In addition to this Neolithic axe Evesham also found a considerable amount of broken painted pottery of the same period. Subsequently he excavated this prehistoric deposit, the first to be found in that part of Greece, and every archaeologist knows his monograph on the subject which was published by the Oxford University Press. Evesham cleared the debris and earth off the skeleton very carefully, since he was particularly anxious to obtain means of identification. The suggestion, of course, was that when the thunderstorm had burst again in full violence, Chubb, terrified as usual by thunder and lightning, had taken refuge in the innermost corner of the basement.

Nothing was above the skeleton and then Evesham began to remove the bones, carefully beginning with the feet. Underneath the heel bones of each foot he found a

semicircular iron boot-heel protector which proved that the skeleton at all events was not Neolithic. By the waist he found the case of a gold watch with a mass of rusted steel inside, and attached to it by a short gold chain was a fob seal, an agate mounted in gold. This he carefully cleaned expecting, or rather hoping, to find Chubb's initials upon it. Instead he found a coat of arms which heraldic experts subsequently identified as those of Lucy, gules, three luces (pike) silver.

'What would Isaak Walton say to anyone who confused luces and chavenders?' scornfully commented Evesham when he received the experts' report.

After he had carefully lifted the skull he found beneath it a double leather saddle bag, ragged and much perished. It seemed very heavy and as they lifted it very gently its rotten fabric gave way and there poured out on the earthen floor a stream of gold coins bursting out of the tattered remains of canvas bags. This delighted Evesham and his workmen who were, like most excavators, tremendously excited by finding gold. He gathered it all up and found there were something like a thousand gold pieces. This he felt must be Chubb, and further exploration in the corner proved him right.

Underneath the saddle-bag lay a pistol in a leather holster. It was difficult to extract the pistol from its case, for corrosion had caused the rusty metal to adhere to the decaying leather. When at last it was released and Evesham sifted the fragments of rotten wood from the bottom of the holster he found three small brass letters, C., F. and G. At last he had proved that this was Chubb and he had solved a mystery of nearly one hundred years' standing. Another similar pistol and holster were later found right against the wall of the basement, where the weapon had apparently fallen from the nerveless hand of the unfortunate Philhellene.

The discovery of the skeleton of Chubb by Evesham's excavations and the solution of the long-standing and romantic mystery attracted much attention at the time. Chubb's bones were reverently buried with considerable ceremony in the churchyard at Gorani, and the Anglo-Hellenic League announced that it would erect a monument there to his memory to record how he had lost his life in the service of Greece. British Philhellene circles were much pleased that it had been proved that Chubb was not a rogue or a renegade and had not stolen the money, but had perished by an act of God, by the explosion of powder ignited by a flash of lightning. The Greeks were also delighted because the honour of Greece and of the Greeks was triumphantly cleared of all suspicion. Here was Chubb! Here was his gold! Neither Petrobey nor his men had murdered, for the sake of the gold he carried, this gallant gentleman who was risking his life for the liberation of Greece. All sides were satisfied. Chubb was duly buried. Evesham was much praised for his acute research, and archaeologists once again began to say that only by correct archaeological methods could such historical problems be solved. Finally, after all the earth and rubbish on the basement floor had been minutely sifted so that nothing could escape, Chubb's gold was carefully collected and taken under armed escort to the National Bank at Sparta and thence transferred with similar precautions to the National Coin Cabinet at Athens.

Later in the year, at the conclusion of his excavation of the Neolithic settlement, the finds from which are exhibited in the Sparta Museum, Evesham returned to Athens and called on the Director of the National Coin Cabinet to enquire about Chubb's gold. He was shown tray after tray of gold coins which had been washed and sorted out according to their kinds.

English guineas, all dated either 1790 or 1813, predominated. There were also louis of Louis XVIII dated 1819, many napoleons of the great Bonaparte himself dated 1812 and about fifty of the rare double napoleons of Bonaparte as First Consul. There were Venetian gold sequins of the last Doge, Ludovico Manin, issued in 1796, and many Austrian

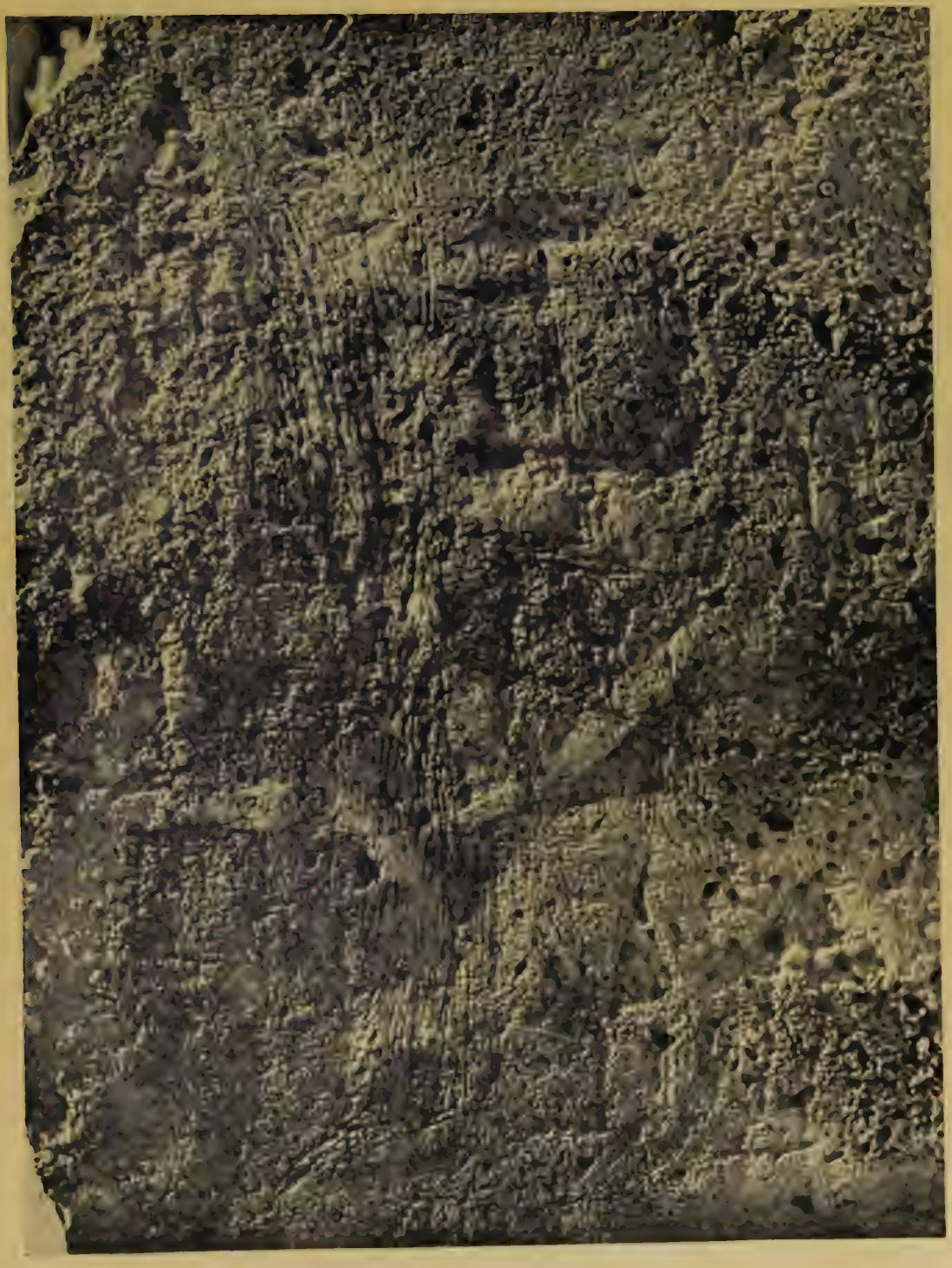
PLATE I

HAL TARXIEN, MALTA : STONES WITH GRAFFITI OF SHIPS.
NOTE ROPE-HOLES IN DOOR-JAMB, STONE A (on left)

PLATE II

SHIP-GRAFFITI AT HAL TARXIEN, MALTA : STONE A
Ph. Valletta Museum

PLATE III



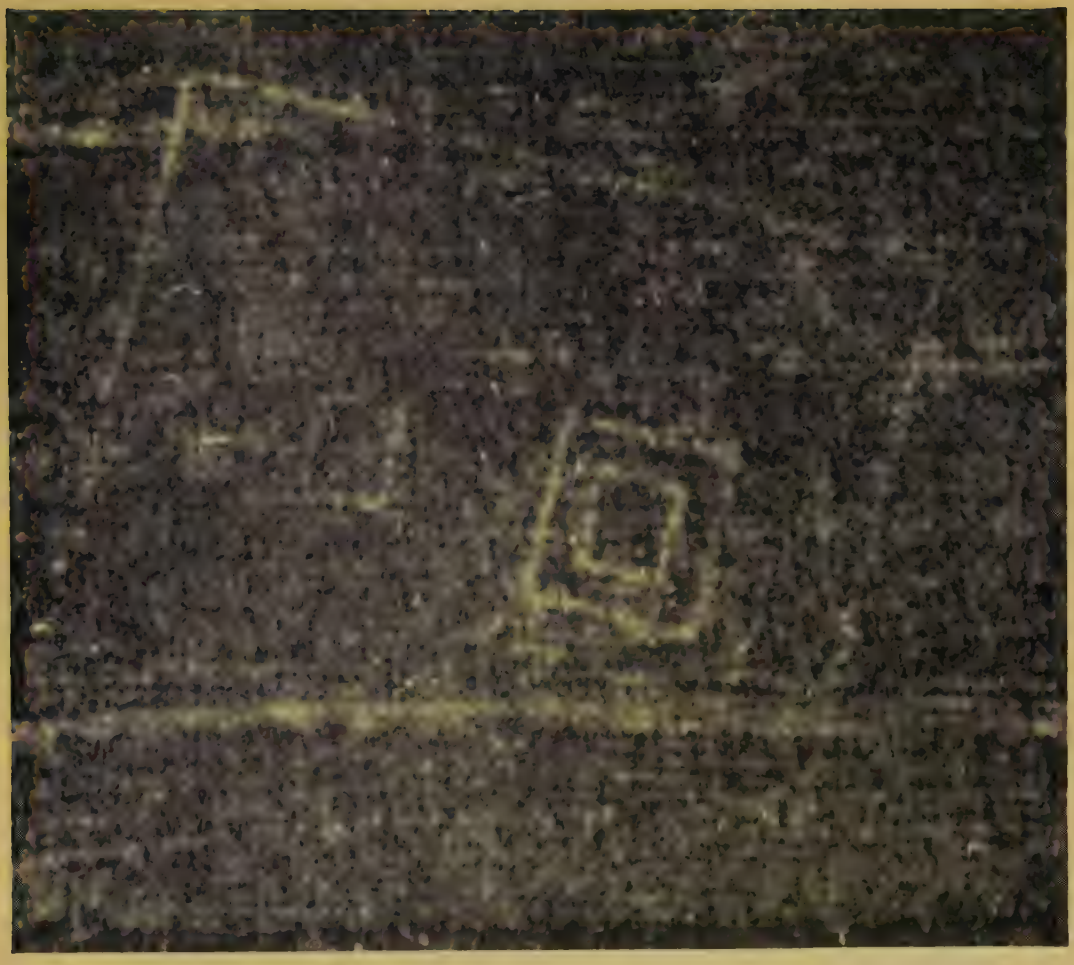
SHIP-GRAFFITI AT HAL TARXIEN, MALTA : STONE B
Ph. Valletta Museum

PLATE IV

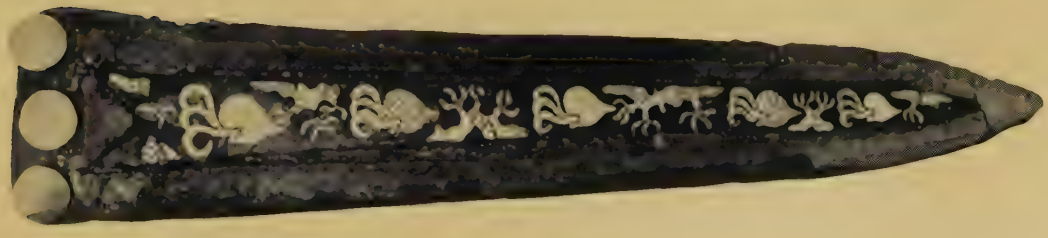
a HOLLOW WAYS AT BOITSFORT, BELGIUM

b. THOLOS TOMB NEAR PYLOS | POT FROM OVER PIT 2

PLATE V



a. AIR-PHOTO OF ROMANO-CELTIC SANCTUARY NEAR FONTAINE VALMONT, CHARLEROI, BELGIUM



b. THOLOS TOMB NEAR PYLOS : DAGGER FROM PIT 2, SHOWING INLAID DESIGN IN GOLD, SILVER AND NIELLO. LENGTH 235 mm.

PLATE VI

a. THOLOS TOMB NEAR PYLOS : DAGGER FROM PIT 2 WITH INLAID DESIGN AND GOLD HILT

b. THOLOS TOMB NEAR PYLOS : ENLARGED VIEW OF LEOPARD IN ABOVE DESIGN

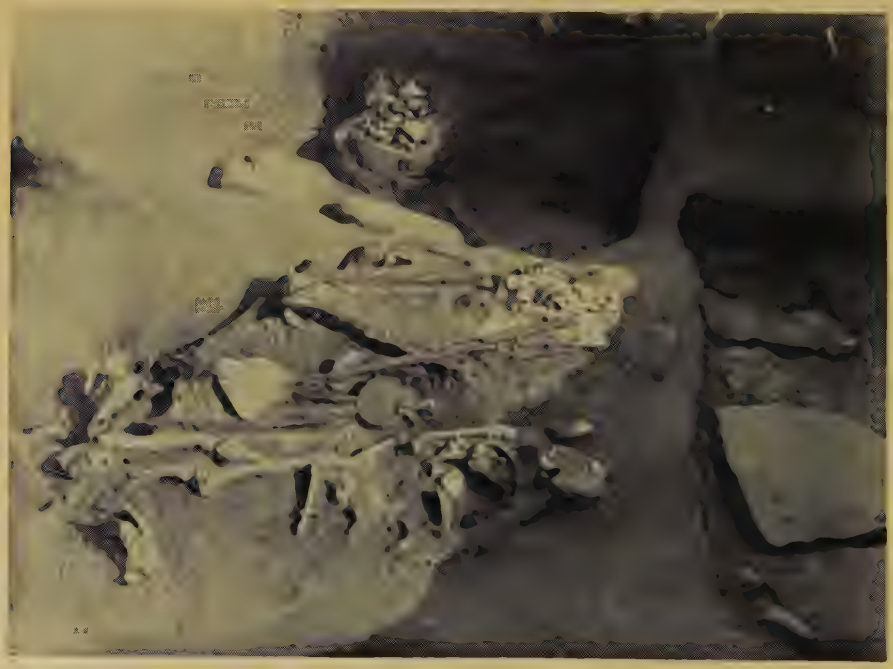
PLATE VII

a. THOLOS TOMB NEAR PYLOS : SEALS AND GEMS

b. THOLOS TOMB NEAR PYLOS : ENLARGED DRAWING OF DESIGN ON GEM (above) SHOWING WOMAN OFFERING LILIES AT A SHRINE WITH SACRAL HORNS AND SPRAYS OF OLIVE ; SKELETON 1

PLATE VIII

a. THOLOS TOMB NEAR PYLOS : ENTRANCE ON S.W. SIDE OF TOMB



b. THOLOS TOMB NEAR PYLOS : IN FOREGROUND, SKELETON 1 WITH ABOUT 10 SWORDS AND DAGGERS, MIRROR, SPEARHEAD AND GEMS ; ON RIGHT, PIT 1 (PRINCESS) ; AT TOP, PIT 2

gold florins of the Emperor Francis I dated 1815. It was remarkable how few currencies were represented in this mass of gold and that the examples of each type were confined to one date only. In all there were exactly one thousand and one gold pieces, a number which naturally provoked some jocular comment.

Evesham was looking over the coins and noting their varieties with the Director when the latter was called away to attend another visitor. When he came back Evesham made some trite remark about its being a nice collection. The Director put on an air of deep mystery and came close to Evesham's ear.

'Would you like to hear a secret?' he asked.

'Of course I would!'

'Ola ine pseftika! [All are false!]' replied the Director in a deep stage whisper.

It was indeed true! He had patiently tested every single gold coin and all were equally false, but obviously the work of some highly skilled coiner. So arose another mystery about Chubb the solution of which must be sought in England. Until his history and his real name can be discovered, for some now assert that Chubb was an alias, we must in all charity suspend judgment. One cannot help wondering, however, whether Byron had not some inside knowledge and so, in calling him Brummagem, was not really making sly allusions about his profession.

Is it after all really conceivable that Chubb was a professional coiner who joined the Greek Committee in order to supply it with his productions? Was he compelled by circumstances over which he had no control to accept a commission to go to Greece with a consignment of his own goods in order to allay suspicion? If so, he never enjoyed his ingenious but ill-gotten gains, for his life was the forfeit.

ANTIQUITY XXXI, 1957

The Campignian Tradition and European Flint-mining

by W. Creighton Gabel

In 1872 the name ' Campigny ' appeared in archaeological literature for the first time, and the industry to which this site gave its name has been a subject of controversy ever since. At Campigny (Seine-Inférieure) were discovered pits containing worked flint and the remains of crude pottery. In a short time similar flintwork was discovered on other sites in north France. The typical implements were axes with the cutting edge formed by a transverse blow and heavy picks. Mortillet was the first to illustrate the analogy between French tranchet axes and those from Danish kitchen middens. In time typologically similar material was found from Britain to the Soviet Union.

Philippe Salmon coined the term ' Campignian ' in his *Dictionnaire des Sciences Anthropologiques* (1882–89), applying it to all these European industries. In spite of the fact that Campigny itself was already assigned to an advanced stage of the Neolithic, Salmon placed the Campignian tradition in the initial stage of the Neolithic, preceding Robenhausian (lacustrian Neolithic). However, it became increasingly clear that Campignian picks and tranchet axes remained in use from the Mesolithic until the dawn of the Bronze Age. Campignian came to be recognized as a vaguely-defined tradition floating somewhere between the Mesolithic and the Bronze Age, and from this compromise sprang the confused literature which followed.

Louis-René Nougier's exhaustive study[1] of the origin and development of this nebulous industry places Campignian at the fore of contributors to the Western European Neolithic. A very brief synopsis of his conclusions is given in the following paragraphs.

The prototypes of the Campignian picks and axes are found in Maglemose and Komsa-Fosna of Scandinavia, but a south Russian source is postulated for such tools. Upon this beginning the ' Pre-Campignian ' was based, and two offshoots appeared in Northern Europe: one in Westphalia and Liège (centred at Aubel, on the Meuse) and one in Scania (centred at Kyvik, near Göteborg). These people were sedentary hunters using pottery.

The ' Classic Campignian ' is closely related to Ertebølle by virtue of the continued use of picks and tranchet axes. These Campignians spread into the Paris Basin, where they lived in villages cultivating cereals and raising livestock. Within the Classic Campignian were a number of specialized groups: forest dwellers, exploiters of the natural reserves of flint and stone, colonists in Britain and Ireland, and pioneers in south-eastern France who came into contact with the lacustrian (Western) Neolithic. To this horizon belongs the ' Thames pick ' of Lower Halstow.

The ' Post-Campignian ' arose out of the contact with Neolithic lake-dwellers. Hereafter, Campignian craftsmen polished their axes and produced new tool types such as sickles and unpolished discoid maceheads.

By this time Campignian proper was overwhelmed by the Western Neolithic, but the Campignian tradition still survived. During the ' Neolithic of Campignian tradition ' the flint mines at Spiennes and Grimes Graves were worked, along with less well known mines in north France. The Seine-Oise-Marne culture is typical of the ' Chalcolithic of

[1] *Les Civilisations Campigniennes en Europe Occidental*, Le Mans, 1950.

Campignian tradition '. The type site for this stage is Campigny, from which the whole complex derived its name. Chassey pottery was added to the ceramic stock, and Grand-Pressigny flint was traded far and wide.

These elaborate hypotheses are difficult to justify. The obvious flaw is that almost without exception Campignian tools are surface finds—whether they be in Ireland, France, or Russia. Consequently, dates are not usually reliable, and in the last analysis our knowledge of Campignian is limited to the heavy flint and stone tools which characterize it.

The source of the tranchet axes seems to lie in the Northern Forest Cultures. There is some evidence that Maglemose or a related group penetrated France and Belgium [2] as well as Britain, and Mrs Hawkes went so far as to describe Campignian as ' a late and poor extension of the Baltic Forest Cultures '.[3] It is now known that Tardenoisian was not entirely without heavy tools,[4] and these people may have obtained what we know of their heavy industry from Maglemosians and transmitted it along with indigenous types, e.g. the *petit tranchet*, to the Neolithic cultures of Western Europe. Tardenoisian sites extend as far as the south shores of the Baltic,[5] and the microlithic bond between Tardenoisian and Maglemose suggests the two groups were in direct contact.

Not all of the Campignian types are so easily placed. The outstanding feature of most of them is their crudity, regardless of their chronological horizon. Indeed, the rough workmanship on Campignian axes at Grimes Graves led to the assumption that the mine was Palaeolithic, until Clark and Piggott [6] were able to show conclusively that such mines were in reality Neolithic. No typological classification of Campignian can survive scrutiny, and furthermore one may assume that in most cases it is the best implements which are selected for illustrations and museum displays.

Campignian cannot, therefore, be considered an independent tradition, much less a population which almost single-handedly forged the character of Neolithic Europe. At most it is the survival of a pre-Neolithic flint-working technique which is only one facet of Neolithic tradition in Western Europe. The cultures in which this Campignian element is strongest are the Secondary Neolithic cultures with their revived Mesolithic heritage and strong flint-mining tradition. A Campignian industry is in fact the common denominator which unites the Secondary Neolithic cultures of Britain, and Piggott discards the term ' Campignian ' altogether and substitutes ' Secondary Neolithic '.[7] The same reasoning may be extended to Secondary Neolithic flint-miners on the Continent, e.g. Michelsberg and Fort-Harrouard.

There seems to be only one logical explanation for the widespread distribution of such crude implements at a relatively late date in European prehistory. These tools are not finished artefacts at all, but only factory roughouts. This assumption appears to be supported by Campignian distribution. As Movius states, ' The bulk of the evidence is from axe factories, workshops, and open-air stations.' [8] Clark noted the association of the Campignian tradition with axe mining.[9] The correspondence between western Continental

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[2] Clark, J. G. D., *The Mesolithic Settlement of Northern Europe*, Cambridge, 1936, pp. 231 and 237.

[3] ' Aspects of the Neolithic and Chalcolithic periods in France ', ANTIQUITY, VIII (1934), p. 26.

[4] Giraud, E., *et al.*, ' Le gisement mésolithique de Piscop ', *L'Anthropologie*, vol. 48 (1938), pp. 1–27.

[5] Clark, *op. cit.*, fig. 65.

[6] ' The Age of the British Flint Mines ', ANTIQUITY, VII (1933), pp. 166–83.

[7] *Neolithic Cultures of the British Isles*, Cambridge, 1954, pp. 277–78.

[8] *The Irish Stone Age*, Cambridge, 1942, p. 211.

[9] *Op. cit.*, p. 161.

flint workings and Campignian distribution is almost exact,[10] and the extension into south-eastern England again coincides with the flint-mining industry. In north-east Ireland the focus of Campignian settlement was in Antrim and Down, where the cretaceous chalk contained the best flint.[11] In this region appeared also the Secondary Neolithic Bann and Sandhill cultures. The same phenomenon occurs at Welsh factories, such as Graig Lwyd, which are assumed to be manifestations of Secondary Neolithic mining activity.[12] Nougier's Pre-Campignian ' Kyvik phase ' in Scania, strangely isolated like Ireland from the core of Campignian settlement, is again in an area of flint exploitation.

Schwantes [13] recognized the fact that Campignian was inseparably tied in with flint-mining and that the primitive appearance of the implements was merely technological convergence resulting in a fortuitous resemblance to Palaeolithic and Mesolithic tools. His postulation was that the use of such crude forms resulted from the abundance of flint, making it unnecessary to waste time grinding and polishing the tools. This may be partially correct (especially in the case of tranchet axes), but probably the majority of the ' axes ' and ' picks ' were never meant to be used in that state.

Pseudo-palaeoliths not unlike Campignian tools have been discovered in North America as well as Europe. W. H. Holmes of the Bureau of American Ethnology made extensive studies in the early part of this century on the association of such products with mining and quarrying in the New World. He was able to show that most represented rejects or ' blanks ' produced in the course of working freshly mined flint or quartzite.[14] A quartzite quarry-workshop near Killarney, Ontario, yielded certain pick-like implements which the excavator felt to be very close to Campignian picks.[15]

That not all Campignian implements are from quarries and workshops does not negate the argument, because most are surface finds or are associated with cultures in which mining was economically important. Possibly some of them represent trade items, for hoards of flint roughouts are often found elsewhere. A certain number may have been used as tools —such as flint picks—in cases where in the unfinished state they would have been perfectly suitable, but few show signs of use. Some of the factory blanks in North America, although indistinguishable from quarry rejects, bear unmistakable signs of use.[16] However, it seems clear that the majority of such flint types in both hemispheres represent nothing more than one phase in the production of flint tools.

Therefore, it would seem that Campignian does not involve a flint-*working* tradition so much as a flint-*mining* tradition in Western Europe which persisted as long as heavy flint implements were produced.

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[10] Nougier, *op. cit.*, map. 15.

[11] Movius, *op cit.*, p. 212.

[12] Piggott, *op. cit.*, p.p. 289 ff.

[13] ' Zur Campignien Frage ', *Germania*, vol. 16 (1932), pp. 177–85.

[14] Holmes, W. H., *Handbook of Aboriginal American Antiquities. Part I: Introductory: The Lithic Industries, B.A.E. Bull.*, vol. 60, 1919, pp. 80–82.

[15] Personal communication with Prof. E. F. Greenman, Museum of Anthropology, University of Michigan.

[16] Bryan, K., ' Flint Quarries—the Sources of Tools and, at the same time, the Factories of the American Indian ', *Papers of the Peabody Mus. of Arch. and Ethn.*, vol. 17, No. 3, pp. 3–6.

ANTIQUITY XXXI, 1957

Wild Doings in the Theological College

by A. R. Burn

THE following narrative is translated from the Greek book of Philon of Byblos (the Biblical Gebal, modern Jebail), who wrote in the reign of Nero. The book is a history of early Phoenicia, down to the time of Solomon, which Philon claims to have edited from an old Phoenician chronicle by a scribe named Sanchuniathon and (as in this part) from other sources. Nineteenth-century scholars were inclined to disbelieve in the existence of Sanchuniathon, and to think that Philon's work was thus a forgery; but now that archaeology has produced actual fragments of early Phoenician literature, including references to a war with the Kerethites of Philistia, which Sanchuniathon also mentions, scholars, especially in Germany, are more disposed to take him seriously.[1]

Even if it is a fiction of the time of Nero, as the prototype of a well-known modern genre it seems to be worth recounting for its entertainment value.

The King Ioram, mentioned at the beginning, seems to be identical with Solomon's friend Huram or Hiram, King of Tyre. An expedition in the Indian Ocean is described just before the story given here, bringing back, as is recorded in the Bible, ' ivory and apes and peacocks ' and other curiosities; an expedition in which the sailors of ' Ioram ' take part with those of ' Irenaeus, King of the Jews '—a name which seems unfamiliar until one realizes that it is simply Philon's translation of Solomon, the ' Man of Peace '.

Sanchuniathon says that, on the death of Ioram, Sydyk his son became king and reigned for forty-two years; and he transferred to Tyre the students from the college which [King] Belîros had founded, because they were misbehaving at Sidon.

For the college which Belîros had founded was near the city of Sidon; and there the sons of the priests were housed and were educated in divinity, and in the law, and in medicine. But in the wardenship of Daêphos, who became very old and easy-going from advancing years, the young men played many tricks, as appears from the book on Sidonian affairs by Bar-Mirhábas, who entered the college at the age of eighteen and studied divinity there for four years.

Now the young men used to receive instruction for ten days in each month; and the other days were allotted to private study and reading. But while Daêphos thought that the students were working hard, and paid little attention to them, they used to go off and spend all their time in town, enjoying themselves. Some were always left behind, so that if Daêphos asked where so-and-so was, they might give an excuse for their absence, saying that Ialykos' father had died, and he had gone to comfort his mother, or that Baliades was ill, and so on, while the rest were making merry in town. Sometimes, too, they used to do so in a house not far from the college, where there would be travelling girl-musicians; for once the girls had met some of the young men there, and received handsome presents from them, and got to know that they could count on it, they never passed by the house, but always made a short stay there before going on into the town.

Among those who used to go into town with Bar-Mirhábas were a certain Hierakon

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[1] For a bibliography of the considerable literature to which the new interest in Sanchuniathon has given rise, see O. Eissfeldt, *Sanchunjaton von Berut und Ilumilku von Ugarit*, in the series *Beiträge zur Religionsgeschichte des Altertums* (Max Niemeyer Verlag, Halle (Saale), 1952).

and Gigas and Charason. Charason was not a student, but the man placed by the king in charge of the salt-trade; but, being of the same age as they were, he often joined in their amusements. They generally used to enter the city at night, in order not to be recognized.

' In the evening ' (writes Bar-Mirhábas) ' we used to go to a tavern used by the slave-sailors, near the harbour, where we used to hear them talking together about the islands and about barbarian lands, for some of them were islanders, and some men of Tarshish, and some from Tengê [in north Africa] and some from Ersiphonia [probably Sicily]. We used to go in after dark, and dressed so as not to be recognized; for in Sidonian society it is considered most unseemly to associate with the slave-sailors. However, we dressed up like slaves, and did, sitting on sacks and wine-skins, of which there were many lying on the floor (for the slaves used to steal lots of things off the ships, and go and drink at the tavern), and drinking the same sour wine as the sailors, for the tavern-keeper did not provide any that was sweet and good. There we heard many fine tales about foreign peoples and places, and we used the sailors' tricks of speech and behaviour, and were never recognized by them, but they thought we were men of the people and of their own class; otherwise they would have thrown us out of the place, for they have the deepest contempt for the gentry.

' However, the proprietor of the tavern was an intelligent man; and he, for the most part by private nods, unseen by the others, showed us that *he* knew quite well that we were not what we pretended to be. But as we enjoyed these parties very much, we promised him, as a bribe not to tell the others, a newly cleaned tunic, and also that we would pay the expenses of a party for all comers. We drank sweet wine all right that time! And Gaphnos (the innkeeper) brought in some girl musicians and dancers, and we kept it up merrily all night. Gaphnos, having kissed one of the girls, got laughed at all round; for his wife saw him and went for him to get him out of that. Now, usually, he was completely under his wife's thumb; but, having been laughed at for being henpecked, he got angry and shouted at her, " Take yourself away. You're all bones, anyhow. I'm not going to sleep on bones tonight." And with that she fled, much abashed.

' The most comic episode was this. A man named Bar-Kêphas, being by now much the worse for drink, said, pointing to his next-door neighbour, " Look at that chap, he can't hold his liquor "; and he had no sooner said it than he turned away his face and was sick himself. Everybody laughed, and when Bar-Kêphas, immediately afterwards, fell into a deep coma, one of the company said, " Look here, chaps, let's have some fun with this fellow—he's drunk. Let's dress him up as Gaphnos and put him on Gaphnos' dining-couch, and when he comes round we will come up to him and call him Gaphnos and treat him as Gaphnos until he really thinks he is the innkeeper. He will be too fuddled to notice anything wrong."

' Everyone agreed to this, and the real Gaphnos slipped away into a side-room where he could see and hear everything that happened in the house. Then the rest did as had been arranged, all with perfectly grave expressions; and Bar-Kêphas, seeing fortune smiling upon him, very soon took the metamorphosis for granted, and began to do everything in the character of Gaphnos. When we began to talk about last night's party he asked what had become of that drunken fellow Bar-Kêphas. We had the greatest difficulty in keeping a straight face at that. Then we applauded him for taking a strong line with his wife; and he said, " Yes, she is very ugly "; and added that he had got a slave-girl, whom he felt inclined to marry as a second wife. This amused us very much. But Gaphnos, hearing all this in his hiding-place, in order to prevent Bar-Kêphas from doing this, sent a private message to a man who had lent him money, saying that Gaphnos, who owed him so much, was breaking his contract, wasting his money on wine and women.

" This very last night," said the messenger, " he has entertained a horde of ne'er-do-wells at a terrific party." The money-lender went straight along to the tavern and, as he did not know his debtor Gaphnos by sight, asked, " Which is Gaphnos here ? " " I am," said Bar-Kêphas; and with that the creditor arrested the so-called Gaphnos and carried him off to the judges at the city gate; and the judges awarded the debtor, as he could not pay up, to be the slave of his creditor until he had worked off his debt. And when he said that he did not think he was Gaphnos, but Bar-Kêphas, they laughed at him and did not take him seriously, and all the rest of us stood round in the crowd and applauded the justice of their decision.

' On one occasion, as we were walking round, we met a troupe of jugglers and snake-charmers, and with them there was a troupe of singers, who told of the death of the giant Iamylkas, who was killed by Sanobal. They had large terracotta images of them, which they displayed: Iamylkas fallen and wounded, at the point of death, and Sanobal standing over him, sword in hand, and exulting in his victory. When evening came on and the audience began to disperse, the men who owned the images wanted to go off too, but in return for a tip they let us have their images and their trumpet. We lit a fire on a stone so as to illuminate the images and began to go through the song in praise of Sanobal in a loud voice. A large audience soon collected. For a time our performance went very nicely, but before long Hierakon cried, " There is the old man coming to look on! We are done for; he'll recognize us." We were in a complete panic. We could not disguise ourselves from Daêphos and we could not run away because of the dense ranks of the crowd. But in this crisis, Charason gave the image of Sanobal a push forward, so that it fell into the fire and smashed with a loud crash; and the flames, which a moment before had been lighting up the whole place, were quenched. For the moment, there was complete darkness; and we made our escape unrecognized, while the bystanders could not make out what had happened.'

All the above is written in the book of Bar-Mirhábas about Sidon.

This, then, was the reason why King Sydyk transferred the college to Tyre, and he built a new college with walls round it nearly fifty feet high to restrain the misbehaviour of its occupants. This building is in the city of Tyre, next to the Temple of Moloch.

Some Recent Books and Articles

The inclusion of a book in this list does not preclude its subsequent review

EARLY ANATOLIA: a description of early civilization in Asia Minor, as revealed by the last half-century of excavating and exploration; by SETON LLOYD. Penguin Books, 1956, 3*s.* 6*d.* [Another of these first-rate Penguins in the series edited by Professor Mallowan.]

ROMAN BRITAIN AND ROMAN MILITARY ANTIQUITIES, by I. A. RICHMOND. Albert Reckitt Archaeological Lecture. *Proc. Brit. Acad.*, vol. XLI; separately for 4*s.* (O.U.P.)

ANCIENT MONUMENTS open to the public: a concise guide to castles, abbeys, houses and other properties in England and Wales, scheduled as ancient monuments by the Ministry of Works, which are accessible to the public. [A useful publication, sponsored by *Country Life* and sold for 7*s.* 6*d.*]

ENGLISH PLACE-NAME ELEMENTS, by A. H. SMITH. 2 vols. Cambridge University Press (for the English Place Name Society), £3 10*s.* [The long-awaited new and greatly enlarged edition of the late Sir Allen Mawer's *Chief Elements*, published 30 years ago.]

STUDIES IN ANCIENT TECHNOLOGY, by R. J. FORBES. 3 vols. Brill, Leiden. 52 guilders.

EXCAVATIONS AT JARLSHOF, by J. R. C. HAMILTON. H.M. Stationery Office, 1956 (received February, 1957), £3 3*s.* [A fine publication; we are publishing a review-article of it in a forthcoming number.]

THE END OF THE ICE AGE, and its Witnesses, by CARL O. SAUER. *Geographical Review*, Jan. 1957, 29–43.

DIE LANGOBARDISCHEN GOLDBLATTKREUZE, by GUNTHER HASELOFF. *Jahrb. d. R.-G. Zentralmuseums Mainz*, 3 Jahrg., 1956, 143–63. [A valuable study well illustrated by plates and excellent distribution-maps.]

ENGLISH HISTORICAL SCHOLARSHIP in the 16th and 17th centuries, ed. by LEVI FOX. O.U.P. for the Dugdale Society, 1956, £1 1*s.*

FIELDS, FENCES, AND GATES, by E. ESTYN EVANS. *Ulster Folklife*, vol. 2, 1956, 14–21. [A useful essay on a neglected subject of considerable importance to archaeologists.]

A STUDY OF THE CRANIAL AND SKELETAL MATERIAL excavated at NIPPUR, by D. R. SWINDLER. Monographs of the Museum of the University of Pennsylvania, 1956. 40 pages, 8 plates, $0.75.

THE HISTORY OF THE BRITISH FLORA, by H. GODWIN. Cambridge Univ. Press, 1956, £4 10*s.* [A review of this standard work will be published later.]

FYNSKE JERNALDERGRAVE, by ERLING ALBRECHTSEN. Ejnar Munksgaard, Copenhagen, 1956. 40 Danish kroner.

Notes and News

EXCAVATIONS NEAR PYLOS, 1956*

The 1956 excavations of the Archaeological Society at Pylos were unusually fortunate in the discovery of a small princely tholos tomb, only partly robbed. The spectacular treasure which it contained gives us grounds to wonder about the contents of those great royal tombs which have been with reason known since antiquity as Treasuries.

The aim of the Greek party in the joint Greek and American excavations in western Messenia has been to gain a general archaeological picture of the area, and this year three places were explored: Tragána, where the previous season's work was completed; Bouphtás (Thuc. iv, 118) (now Boïdokoiliá), by the sea; and Roútsi, farther inland, near Myrsinochórion.

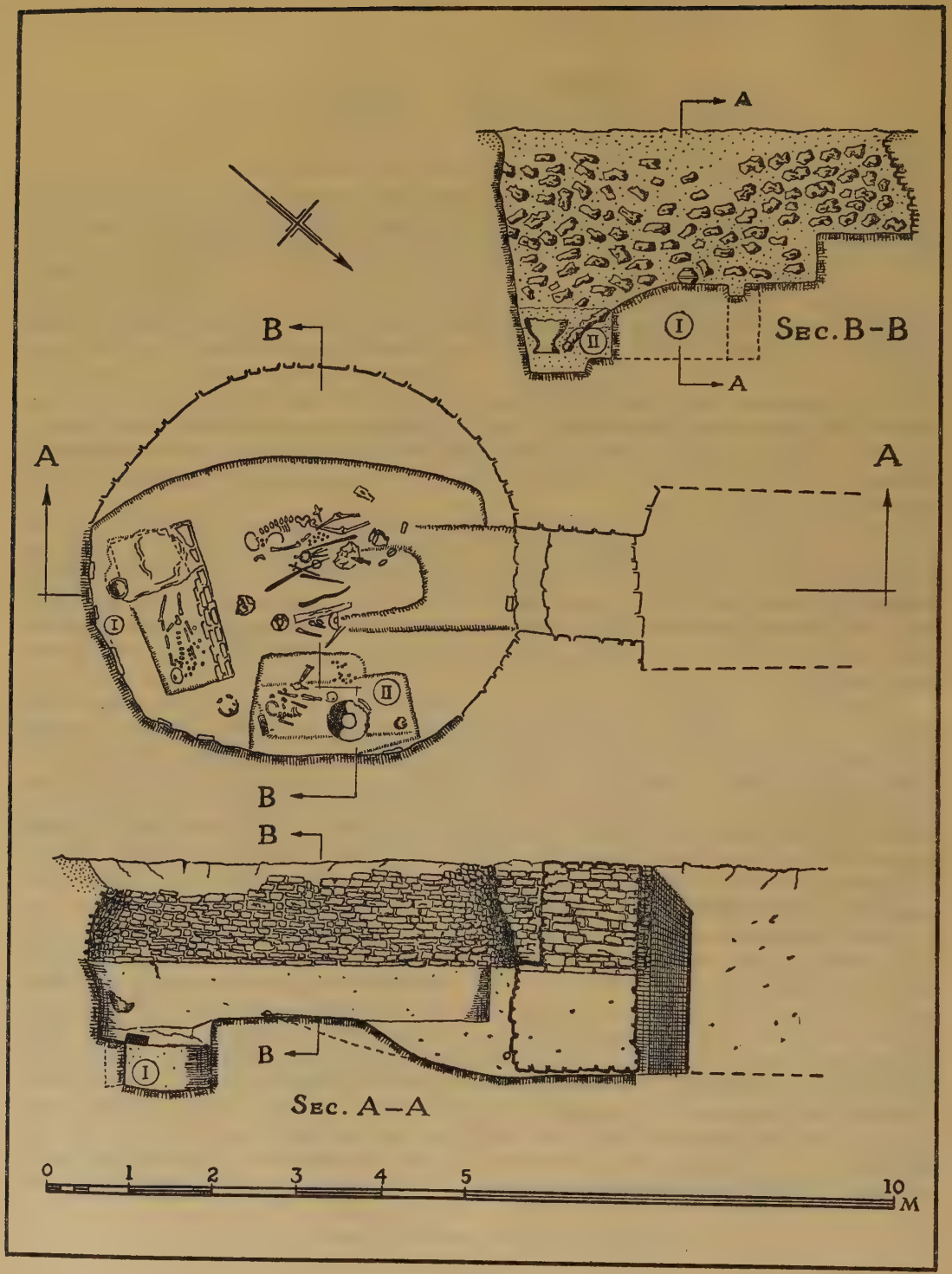
At Tragána the investigation and plans of the two princely tholos tombs were completed. The larger and finer of them, Tomb 1, yielded some good Palace Style sherds, a gold disc and some minor objects, notably sard beads. Characteristic are the two ruts in the floor of the entrance which facilitated the entry of the chariot or cart on which the body was carried.

At Boroúlia the 1955 excavations were completed and it was shown that the site involves an isolated one-roomed building in which had been stored groups of vases and triton shells and nothing else. The significance of the find lies in the unique collection of pottery and its importance for chronology. All belongs to the earliest phase of L.H. 1. Most of the vases are hand-made and coarse in fabric, but one group comprises characteristic cups of the Keftiu type and there are two matt-painted vases purely Middle Helladic in technique.

About 5 km. to the south lies a small cove named, from its shape, Boïdokoiliá, though the name has been altered to Bouphtás by the Tourist Office at Pylos. Entering the cove, on the right, is the cave of Neleus; on the left of the entrance to the harbour a small tholos tomb was excavated. It was erected on the solid rock, entirely above ground. A large Hellenistic ditch had cut through the tomb and destroyed its walls, but on either side were found two skeletons *in situ*, a small rectangular tomb, by it the remains of the sacrifice of a whole bull and some skulls from earlier burials. The finds comprised a mass of obsidian and flint arrowheads, two necklaces of amethyst and sard, four gold bands with spiral decoration, a few minor objects of gold and other materials, and two small late vases.

More important were the two other tholos tombs of Myrsinochórion, at Roútsi, about 1½ hours N.E. of the palace at Englianós. As at Bouphtás they were small princely tombs with an average diameter of about 5 m. Other indications lead to the conclusion that there were both kings and minor local rulers, the *βασιλῆFες* and *λαFαγέται* of the Linear B tablets from Pylos. Of the Myrsinochórion tombs one had long been known and used as a shelter for fattening swine. It is well preserved and only the uppermost part of the tholos had collapsed. In the dromos a slab-covered grave was opened; it was probably Early Christian and contained no offerings. The skull of the body in it bore a wound on the occipital bone and the head itself had been cut from the body and set askew upon the shoulders. The interior of the tholos had been almost completely plundered and on its floor was found a Late Byzantine glazed sherd. No burial was found *in situ* but there were

* Translated by John Boardman, January 1957.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two groups of small vases, one in the northern part of the tholos with L.H. II vases and the other in the south, with L.H. III vases. Near the latter was a well-preserved bronze vessel shaped like a frying pan, and within it fragments of a silver vessel with bones of a lamb or kid, the funeral feast. A gold band, gold leaves, gilt beads, a bronze pin and some other objects were found scattered in the tholos.

At 20 m. away to the S.W. was found Tomb 2. It was much less well preserved, but from the first there were indications that it had not been disturbed. Near the surface appeared fragments of the skull and bones of a calf, Hellenistic sherds and fragments of a large amphora in advanced Palace Style; all these finds lay above the tholos, whose stones were apparent for half of its circumference, the other half being destroyed. In the upper levels of the dromos and before the walled door, whose lintel was not preserved (PLATE VIII*a*), were found a fragment of a gold band and many shreds of the finest Palace Style vases.

In the lower levels of the tholos only stones were found until the floor was reached. It appeared that the tomb contained one body on the floor as well as two pits covered by stone slabs (PLATE VIIIb). On the floor in the southern part was a sort of bench of earth, and between the two pits was another bench or basis, triangular in shape, on which was found *in situ* a plastered table of offerings decorated with an octopus painted in red.

That the tomb was practically undisturbed has been remarked already. The story seems to be as follows. The number of skulls recovered suggests that there were six burials, after which the tomb fell into disuse, probably because it had collapsed. Pit 2 held the earliest burials with at least four skulls, but they were in such a poor stage of preservation that nothing more detailed could be learned about them. Only the uppermost body was *in situ*, the earlier ones having been repeatedly disturbed and the offerings removed. The vases had been broken and scattered in the tholos so that of the forty odd vases in the tomb only one small askos was found undamaged while all the other vases lack small fragments. The latest burial was that found on the floor of the tholos. The body lay at full length with its right leg over its left and both arms stretched to the right-hand side of the body. The offerings were all *in situ*, but the collapse of the tholos had broken them as well as the skeleton, especially its skull and ribs. The body had around its neck a heavy, amber necklace, some fifteen seal stones in its hands (among them an Oriental cylinder of sard, and another of sardonyx in Mycenaean style), ten swords and knives at its right side, a flesh-hook at its left and vases at its head and feet.

Pit 1 contained the skeleton of a young princess, about fifteen years old. A large necklace of spherical beads in polychrome glass was preserved around her neck, and a glass prism imitating amethyst was found by her right arm; there were no other offerings.

Pit 2 was secured by the three great stones which hid it. On them there stood upside-down a large three-handled amphora in superb condition (PLATE IVb). Of the uppermost body only the top half could be clearly distinguished. A large necklace of amber beads covered its neck and breast. By the left hand lay a dagger inlaid with alternate gold and silver nautili on a niello background and with marine decoration in the same materials (PLATE Vb). A little beyond it lay thirteen gold beads, in shape and size like shells. There can be little doubt that they were from the belt which held the dagger.

A similar dagger, in a corroded condition, soon came to light under the body's right armpit. It had a gold hilt and inlaid on the blade wild beasts issuing to hunt in a landscape of streams, rocks and trees (PLATE VI). At the left-hand side of the body lay a small mirror, and on it another gold-riveted dagger with a gold ring at the end of its hilt.

The remainder of the offerings, which belong to earlier burials, were scattered in the pit. The most notable are bronze mirrors, weighing scales, a spear, a tripod and a flesh-hook; scraps of ivory; a gold ring without its bezel, a gold ear-scraper and various gold

leaves; a silver cup, and a gold and silver pin of fine workmanship with an amethyst head; two seal stones with representations of a griffin and butterflies, two amethyst prisms (one set in gold) with a deer, a lion and flying ducks; a remarkable sardonyx set in gold with a griffin, and a solid gold bead seal carrying the scene of a wild-bull hunt with nets (PLATE VII).

A small ivory comb was found in the pit and another in fragments by the door of the tomb. This bore a remarkable representation of felines stalking ducks. Also near the door were two ivory pyxides, one of them with a representation of dolphins, the other with spiral decoration. Finally, dozens of stone arrowheads were found in the tomb and dromos, as is usually the case in Pylos. It is significant that these were not left as offerings, but seem to have been fired while the burial was in progress as salutes in honour of the dead and perhaps to frighten away evil spirits.

The pottery from the tomb has not yet been fully studied, but the more important vases have already been mended. They range from the late Marine Style (in Crete, L.M. I B–C) to the advanced Palace Style, but there is nothing later. For dating we may say that the tomb was built a little after 1500 B.C. and went out of use before 1400 B.C. According to Furumark's new classification the pottery is Myc. II A–III A, 1. Some half-dozen vases I believe to be genuine Cretan imports, especially a stirrup-vase which, with another in Chalcis Museum, is the earliest example of this shape in Mainland Greece.

S. MARINATOS.

HOLLOW WAYS AT BOITSFORT, NEAR BRUSSELS

Modern construction, in a region as densely populated as north Belgium, has obliterated most vestiges of the past. Therefore it is very pleasant to find near Boitsfort, a fashionable suburb of Brussels, just a short ride from the centre of town, part of a great state forest where some traces of antiquity do survive. The Forêt de Soignes, its floor kept meticulously clear of undergrowth by the foresters, is a very agreeable place for field work at almost any season of the year. The old, tall, stately trees interfere little with vistas of rolling sand-hills covered most of the year round by a golden leafy carpet. Shadows cast on clear days by the great trees serve to bring out any undulation in the ground, and these features photograph with startling clarity (PLATE IVa).

Just south of the race track at Boitsfort, a deep series of gullies in the forest marks the beginning of the trace of an old road running for several kilometres to the south-east. FIG. 1 is a sketch map, traced from an air photo made in the winter showing the course of this old track. The deep ravines in the easily eroded sandy loess produce a snarl of forms which have often led to confusion concerning the true nature of the site. PLATE I shows an especially deceptive portion of the road where the erosion ditches bear a startling resemblance to the banks and ditches of a hill fort. Indeed, this particular part of the road has been interpreted as such. It is marked with an arrow in FIG. 1, and a detailed plan of this portion of the road, made in 1955, is shown in FIG. 2.

The hollows are unequal in number, two running on the western half of the modest promontory and four or five on the east. The lines of the hollows do not close, but they simply peter out at both ends. Since the northern part of the hill is nearly level, this system would make a poor defence if the site were a fort as supposed. Gerhard Bersu remarked to the writer when we visited the spot that the tops of the ' banks ' lie level with the natural contours of the hill and that the bottoms of the ditches are eroded below them. Some places show clear signs of recent disturbance and out-wash. The site has been confused in the literature with a worthy Neolithic settlement half a kilometre east; a site which presents no visible above-ground traces having anything to do with the Neolithic period.

Thus Boitsfort and its ' levées de terre ' has passed erroneously into the literature as a fortified Neolithic settlement site. A commission organized in the early twenties found that the ' banks ' were composed of ordinary sandy sediment in undisturbed geological sequence, but this report seems little known.

Similar hollow way systems abound in the forests of northern Europe. Some excellent examples can be seen in the Taunus mountains paralleling the road from Neuwied to the Altenkirchen exit of the Cologne–Frankfort autobahn. Here the brown earth soil base is

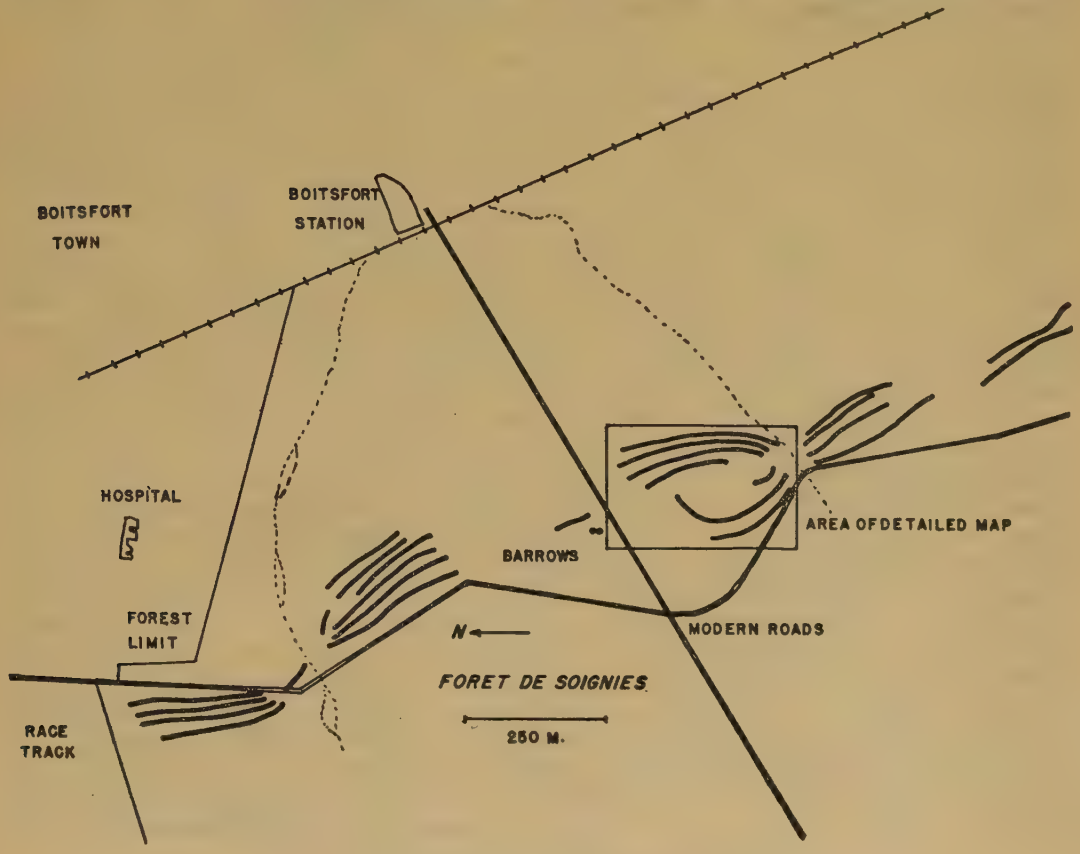

Fig 1. COURSE OF THE HOLLOW WAYS AT BOITSFORT

not as deeply gullied as is Boitsfort even though the slopes are much steeper. A magnificent set of hollow ways in a wood, three on each side of a steep drop descending to a paved road of late Iron Age date which crosses a bog, can be seen near Broskov, Praestø Amt, in Denmark. The overall plan there resembles Boitsfort, creating the impression of a fortified hill, but the banks are at natural contour and the ends do not close either. The paved section, excavated in recent years, proves beyond all doubt that the hollow ways connect with a road of prehistoric date. Subsoil here is glacial till, and its lower mobility has prevented the formation of extremely deep ditches. The average depth at Broskov is less than two metres as compared with three and more in the Forêt de Soignes.

The Editor of Antiquity has suggested that a hollow way begins to form on a track

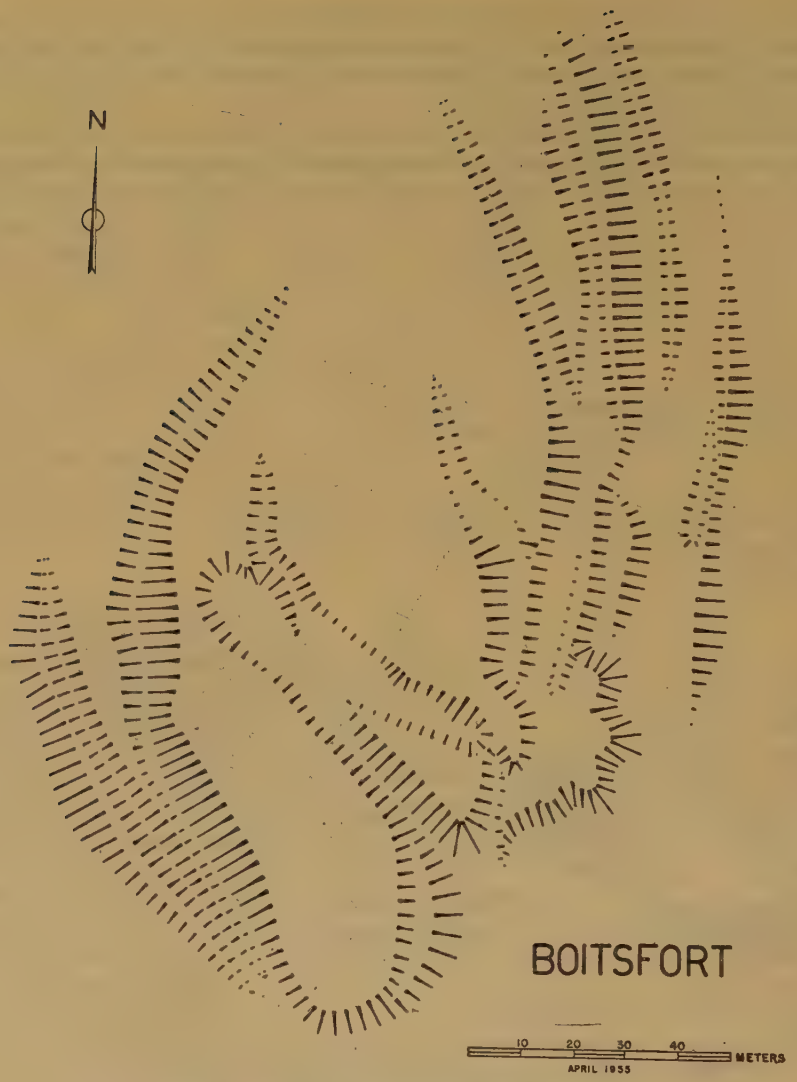

FIG. 2. DETAILED PLAN OF HOLLOW WAYS SEEN ON PLATE IVa

as the result of erosion after the turf has been destroyed by traffic. It seems likely that the humus horizon of the soil must be opened so as to expose the more mobile lower layer (the B horizon of the pedologists) more or less completely. Further, the break must be maintained by continued traffic so that the humus layer cannot reform. When the humus does reform with the abandonment of the route or a great reduction in traffic, the hollow way gradually ceases to erode and ' fossilizes ', held in place or nearly so, by the roots of the undergrowth or by grass. The proviso is of course that the slope is not so great that continued erosion finally produces a veritable ravine.

The hollow ways in the Forêt de Soignes clearly antedate the present crop of trees planted there some hundred and fifty years ago. They are often right in the middle of

the track. Considering that the depth of the humus horizon in the hollows is much the same as that in the undisturbed parts of the forest (observable in cuttings), it seems reasonable to suppose that the system is at least as old as the forest itself. The first records show that the Forêt de Soignes is mentioned as a royal preserve in the 9th century. The presence of several large barrows along the course of the track may indicate an even greater age. These mounds were dug early in the century with no result, but the traces of the old cuttings show that only a smallish pit was let into the centre of each. Given the early crude excavating technique and the destructive nature of sand, the likelihood that such a cutting would find anything is small enough.

The depth of hollow ways seems to be no indication of age in any general sense. It appears to be largely dependent on basic soil mobility and on the degree of natural slope. Hollow ways joining the Bavai–Cologne Roman road at regular intervals are of very great depth indeed. The road passes for the most part through highly mobile loess where wind as well as rain erosion may play a rôle. Here in the southern part of Belgium the great depths of the hollow ways (over 5 m. in some cases) can be attributed to this great mobility as well as to the continued use of the roads, in spite of the slight slopes involved. Hollow ways in sand seem not to be quite so deep, while those in brown earth or glacial till are still more shallow, even when the slope is comparatively great. Since many are roughly comparable in date it would seem that depth is more dependent on soil type than on age.

It appears that for any given soil there is a minimum slope which is necessary for an ancient track to leave hollow ways as a trace of its passage regardless of age, just like there is a fixed angle of slope for stability of a pile of any given soil. Further, since the formation of hollow ways depends mostly on water erosion, a moist climate is more likely to lessen the minimum necessary slope angle. Thus there seems to be a historical variation in the criterion of minimum necessary slope following changes in average rainfall. This variation is probably small since Roman times, but it might have to be allowed for when considering the effect of soil type. The volume of traffic is probably significant only in so far as it is necessary to maintain a break in the humus horizon, further traffic having but slight effect in the given hollow. Of course heavier traffic is likely to be responsible for multiple ruts. An increase in traffic above the critical amount can certainly tip the situation in favour of hollow way formation. Abandonment of the route or decrease in traffic below the critical point probably leads to a slowing down of erosion, a silting up of the lower ends of the ways, deepening of their centres for a time, and then ultimate humus reformation, which more or less stabilizes the system and preserves it at some depth less than that which it achieved before going out of use. We find the roads in the Forêt de Soignes and many other hollow ways in just such a ' fossil ' condition.

IRWIN SCOLLAR.

A ROMANO-CELTIC SANCTUARY IN BELGIUM

The air photograph (PLATE va) shows crop marks on high ground south of the river Sambre near Fontaine Valmont, Hainaut, which lies about 20 km. south-west of Charleroi. The photograph is one of a series obtained in the early summer of 1955 following reports of the discovery of Roman pottery and large blocks of dressed stone in a field close to the site to be described. A visit revealed that the blocks, removed from their original position by the farmer, were clearly from a substantial building of uncertain period. Inspection of the field disclosed a scatter of Roman potsherds.

The crop marks, which appeared in a neighbouring cornfield, I discovered when the photographs were handed to me for examination. I provisionally identified the markings

as revealing a Romano-Celtic sanctuary. The main feature visible is a rectangular enclosure with sharp angles. The east and west sides are 67 m. in length, the north and south 49 m. Two internal structures can be seen. These are in alignment with the sides of the enclosure but lie nearer to the west side than to the east. The southern, larger building is composed of two rectangles, the inner about 8 m. from east to west and about 7 m. from north to south, the outer about 16 m. by 15 m. The northern structure is a single rectangle of approximately 8 m. by 7 m.

The identification of the site as a Romano-Celtic sanctuary seems reasonably certain on the grounds of plan alone. The southern building is of the box-like type familiar as a temple design in the northern provinces of the Roman Empire. The enclosure of one or more temples by a precinct wall is not uncommon, especially in the countryside as here. The placing of the buildings away from the main axis of the enclosure is often encountered on such sites. It may be due to the presence of sacred groves or other features,[1] possibly pre-existing. However, it is usually more pronounced than in this instance, where it is perhaps accidental.

The interpretation of the northern structure is more problematic. The close correspondence of the sanctuary at Gergovia in other respects[2] suggests that it may provide a parallel for this also. At Gergovia the continuous stone foundation of the veranda wall of one of the pair of temples was replaced in the other by individual bases for pillars. These must have served as bases for the pillars which supported the outer edge of the veranda roof. Such substantial blocks might well have been removed at Fontaine Valmont by cultivators or stone-robbers or may simply not show up on the air photograph. Alternatively, the whole of the veranda structure may have been in timber, which would not necessarily produce a crop mark at the same time as the stone foundations.

There is no sign on the photograph of an entrance to the enclosure. Nor is there a trace of porticoes along the precinct wall as are found in several other sanctuaries, including Gergovia.

Excavation of part of the southern building was carried out by the Director of the Musée de Mariemont and Mr Irwin Scollar in August and September 1955. Pending the publication of the report, it is only possible to state that the Roman date of the building was confirmed and that constructional details were revealed consistent with the identification suggested. A point of technical interest is that it was possible to plot the data from the photograph on to a map with only a very small margin of error. The fact that the excavations have so far confirmed the existence of the features plotted demonstrates the possibility of preparing reasonably detailed and reliable plans of unexcavated sites from air photographs. Moreover, the consequent ease with which the excavation-grid was sited for the maximum advantage shows the value of the method in the actual laying-out of an excavation.

P. SALWAY.

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[1] I. A. Richmond, *Roman Britain*, 1955, p. 194.

[2] O. Brogan and E. Desforges, ' Gergovia ', *Archaeological Journal*, XCVII, 1940, pp. 18ff.

Reviews

AN INTRODUCTION TO ANGLO-SAXON ENGLAND. *By* PETER HUNTER BLAIR. *Cambridge, at the University Press*, 1956. *pp.* xvi + 382. 30*s. net.*

The plan of this book is unusual, but Mr Hunter Blair tries to disarm criticism by calling it not a history of England in the Anglo-Saxon period, but an introduction to the country in that age. The book consists of six chapters, each rather irritatingly divided into numbered sections for the benefit of readers who may lack sufficient concentration to follow a closely knit narrative. The first two chapters, ' The Foundations of England ', and ' Britain and the Vikings ', cover the political aspects of the long age between the last days of Roman Britain and the Battle of Hastings. Since this part of the book takes up only 115 pages, the author has not been able to allow himself space enough to bring out effectively all the significant features in the politics of the period. Moreover, his own interests seem to lie in the earliest centuries, to which an undue amount of his space has been given—in comparison with the meagre treatment accorded to Offa, to the effects of the Danish settlements, and to the end of the Old English state. Not all historians will feel able to follow Mr Hunter Blair in his argument that the water-logged country between the Humber and the Pennines was in all probability the march from which Mercia took its name. In the absence of any definite evidence there seems no reason to abandon the traditional view that the march between the English and the Welsh, lying as it does close to the heart of the Mercian kingdom round the upper waters of the Trent, gave its name to the great Midland kingdom, which in the 7th century comprised 5,000 households to the south as well as 7,000 households to the north of that river. It is not helpful to be told that the settlement of the Danes in Yorkshire and the consequent collapse of Northumbria ' marks the beginning of a period of great political confusion in the north which lasted throughout the Middle Ages and which indeed was resolved only with the union of the English and Scottish crowns in the seventeenth century '. The strong medieval English kings, who sent judges on circuit through the North and collected a national taxation from the northern as from the southern shires, would hardly have agreed with this. It might, perhaps, have been well to modify the somewhat misleading statement that ' it may well be that a given (place) name which is first recorded in writing as late as the eighteenth or even the nineteenth century can be confidently ascribed to the Anglo-Saxon period and even to its earlier part '.

But Mr Hunter Blair's book will be judged less by his account of the gradual building up of the Old English kingdom than by the value of the chapters in which he deals with ' The Church ', ' Government ', ' Economy ', and ' Letters '. In dealing with each of these great subjects in a single chapter for the whole period he was setting himself a difficult task, and in the end the book remains rather a collection of long essays than an integrated unit. For that matter, the period with which he deals was not itself integrated. It began in mists, and was broken in the middle. Mr Hunter Blair's attitude to the man whom he, like all others, regards as the chief guide through the mists invites more than a casual question. A scholar like Bede, moved by a religious purpose to attempt an ecclesiastical history of the English people, founded on information from every available source, should not lightly be abandoned on account of an assumed prejudice when he criticizes the British clergy for their abstention from the work of converting the heathen English. The treatment of the growth of scholarship as a separate subject from the Church seems to weaken the earlier chapter and to deprive the later one of something of its purpose. It is perhaps

unkind to complain about the pictures, but they form a tiresome break in the story when, as in the case of the page from the *Parker Chronicle*, the page from the Moore manuscript of Bede, the front panel of the Franks Casket, and the Kirkdale inscription, they necessitate a facing page of description. To the reviewer the most interesting of all the chapters is that entitled ' Economy ', with its attempt to bring back to mind the countryside of those remote times and to make the complications of the early English coinage understandable to the modern reader. DORIS M. STENTON.

ASTRONOMICAL CUNEIFORM TEXTS. *By* O. NEUGEBAUER. *Lund Humphries (for the Institute for Advanced Study, Princeton, N.J.)*, 1955. 3 *vols. pp.* 511 + 254 *plates.* £5 5*s.*

Professor Neugebauer, a well-known historian of mathematics and an experienced reader of Akkadian texts, has greatly advanced our knowledge of Babylonian astronomy by publishing more than three hundred inscribed clay tablets dated in the Seleucid era (*c.* 240–40 B.C.). While the majority of these documents are thus made available for the first time, he has taken care to republish all the records of the same class made known since the pioneer efforts of Kugler and Epping in 1881, when they identified the general character of these texts in the Babylonian collections of the British Museum. The documents come from Babylon and Uruk (also known as Warka or Erech); they are lists or tables of the position of the moon and planets computed for regular time intervals (' ephemerides '). These were used not merely to find the place of any heavenly body at a given time but to predict phenomena such as new moons, last visibility and eclipses. The texts are, then, the early precursors of our modern Astronomical or ' Nautical ' almanacs. This work also includes some texts which provide the basic calculations for various columns of the ephemerides and a few ' procedure-texts ' which reveal the predominantly mathematical methods and sexagesimal system employed. The commentary on the texts is arranged according to the principal subject, i.e. the moon (Vol. I); the planets, in modern order (II), followed by transcriptions and photographs of the texts (III).

This is a standard work in every high sense of that term. It provides a text-book, bibliography and lexicon for the Assyriologists, few of whom are acquainted with this field of study, and a series of mathematical tables, computations and problems for the advanced astronomer, few of whom will be able to criticize this book. No attempt is made to give an introduction to ancient astronomy for the general student, for this has been done elsewhere by the same author,[1] so that the following brief summary may help to give some idea of the importance of these texts.

The preliminary stages of Babylonian astronomical science are as yet but little known, though it is now clear that by *c.* 400 B.C. the fixed rule of intercalations over a ' cycle ' of nineteen years had been adopted. This step in controlling the Babylonian lunar calendar led to the development of theoretical astronomy, for it necessitated the prediction of the length of the lunar month as calculated from the visibility of the new crescent. For centuries the Babylonians, who had recorded detailed observations of the planets—as of all nature—were aware of the insecurity of direct observation. Now Neugebauer is able to show us the very tables and calculations which resulted from the new learning of the 3rd century B.C. This same information could have been known to the Greeks following

[1] ' The Exact Sciences in Antiquity ' (*Acta Historica Scientiarum Naturalium et Medicinalium*, IX), Copenhagen, 1951; ' Ancient Mathematics and Astronomy ' (*A History of Technology*, I, pp. 785–803), 1955.

Alexander's conquest of Babylon; it was certainly incorporated into their astronomical systems by 150 B.C. (Hipparchus).

These texts will not directly further the knowledge of the chronology of the Seleucid period, which is otherwise well established [2] in comparison with earlier periods of Babylonian history. They do help, however, to underline the accuracy of Babylonian astronomical methods which have sometimes been called in question when brought forward as evidence in discussions of the chronology of the ancient Near East.

D. J. WISEMAN.

EXCAVATIONS AT SOBA. *By* P. L. SHINNIE, F.S.A., *with a section on the glass by* D. B. HARDEN, Ph.D., F.S.A. *Sudan Antiquities Service: Occasional Papers No.* 3. *Khartoum*, 1955. *pp.* 84, *with* 30 *plates and* 47 *figures.* 50 *P.T. or* 10*s.*

Medieval Soba was on the right bank of the Blue Nile some thirteen miles above Khartoum. The site is littered with broken bricks and potsherds, and though there are more than a hundred mounds of debris there is nothing in the shape of building fragments to indicate what lies below them except in one place where there are three columns poking out. Budge, who dug here a little in 1903, found a Coptic cross on one of the colums and reports that ' the remains of the side buildings of several of the city gates were considerable ' (*The Egyptian Sudan*, vol. 1, p. 42), but no one else has mentioned them. Bishop Gwynne tells me that he remembers being shown bricks from Soba in the ruins of Gordon's palace, and the site had been ransacked for building material in the time of the Egyptian Government. Treasure hunters had also been at work; there is a useful list of antiquities found, or supposed to have been here, in this paper (pp. 15–17). A large site—it covers about a mile in area—with a history of this kind is not the kind of place which tempts scientific excavators, and Mr Shinnie is the first archaeologist to take up the challenge seriously; he, I regret to add, had had enough of it at the end of a second short season in February 1952.

By that time he had excavated in three mounds towards the middle of the site and dug two trenches against the ' church '; plans and sections of the work had been made, but much remained unexcavated in each place, as he very frankly admits.

In the larger mounds, A and B, the accumulations were about 4 m. high; they consisted of the remains of houses superimposed, with some striking changes in orientation, on at least three levels. Red bricks were lying about, but the standing walls were of unbaked bricks; one is shown on the section nearly 3 m. high. Some fair-sized rooms were found; on the floor of one, which measured 10·5 by 9·5 m., there were four bases for pillars, probably of brick, to support the ceiling. Some scraps of painted wall-plaster were found in mound B, but not enough to identify the pattern, and fragments of glass window-panes in both the larger mounds and the ' church '. The pottery included some fragments of glazed Arab wares; four of these were assigned to the 10th century by Mr A. Lane of the Victoria and Albert Museum, two to a Fayum fabric and two to a Mesopotamian, all from mound B, level 2. The large collection of glass fragments was submitted to Dr D. B. Harden; they are difficult to date; the main parallels are some surface finds from Aydhab which Dr Harden publishes as an addendum to his catalogue and discussion of the Soba material, and he feels that his provisional recognition of two groups—the first a Byzantine to Early Islamic group of the 6th–9th century, the second probably Islamic of the 9th–12th century—' may have to be revised '. It may be worth adding that much glazed earthenware

[2] A. J. Sachs and D. J. Wiseman, ' A Babylonian King List of the Hellenistic Period ' (*Iraq*, XVI, 1954, pp. 202–211).

and glass was also found in 1907 at Badi, another Red Sea port, and placed in the Khartoum Museum with three inscriptions from the same site bearing dates A.D. 997, 1015 and 1037 (*Geographical Journal*, May 1911, p. 545).

The plan of the ' church ' is still obscure and the account of it is sketchy; the floor is mentioned twice, but we are not told what it was like nor how far it stretched, and there is a reference to ' various broken columns and capitals lying about the area ' but no indication of the number of columns or the character of the capitals. Can the ' church ' have been a small square erection like those recently published by Dr Crawford, or did it extend farther north or west where no trenches were dug, and were the capitals like those moved into Khartoum in 1929 (see p. 17) or plain blocks like the bases under the columns that are still standing ?

Neither on the church site nor in the mounds were the excavations carried quite far enough. In the mounds, owing to the layout of the work—a grid of 5 m. squares separated by 2 m. paths, adopted the writer says (p. 79) from one of the layouts in *Ancient India*—the plans of the houses are all incomplete and we do not know whether they opened on streets or stood inside courts, and the rooms are almost all more or less truncated. The culture is not at all primitive; it is what we should expect from the descriptions of Soba in medieval writers as the rich capital of a rich Christian kingdom, and we want more details.

Apart from these shortcomings I have no criticism to make; the notes on the history are good and the illustrations of the pottery and other small finds, which are mostly the work of Mrs Shinnie, are excellent. A real beginning has been made.

J. W. Crowfoot.

EXCAVATIONS AT HASTINAPURA AND OTHER EXPLORATIONS IN THE UPPER GANGA AND SUTLEJ BASINS, 1950–52. *By* B. B. Lal. *pp.* 151, 73 *plates*, 37 *figs.* *Ancient India, Bulletin of the Archaeological Survey of India*, Nos. 10 and 11, 1954–5. Price (complete number) *Rs.* 9 or 14*s.*

The Archaeological Survey of India can, with some justification, regard this report with a considerable measure of pride. Firstly the reader is left in no doubt about those important points of ' why ' and ' where '. This dig was undertaken to extend our knowledge of the once blank and still rather hazy period between Harappan times and the start of the Mauryan dynasty (*c.* 1550–324 B.C.), and in this purpose it has succeeded. There are maps drawn to four scales which pin-point the site precisely. The method of excavation and reasons for it are clearly stated, strata fully described and there are two very well-drawn sections. The objects recovered are well described and clearly related to the plates and line-drawn figures; in fact, cross-referencing, so commonly shirked, is impeccable. The photographs of structural remains are clear and human figures are introduced intelligently. If anyone thinks that the above points are so routine as scarcely to deserve praise, much evidence is available during the last thirty years to prove him wrong. There is, however, one definite criticism. Information could have been presented with regard to the quantitative distribution of important pottery types and techniques by means of a table, showing by levels the number of sherds recovered of painted grey and other wares in Period II, and northern black polished and other wares in Period III.

With regard to interpretation, it is necessary to indicate that the chronology, which has been stated with some assurance by the excavator, is open to considerable criticism. There is little doubt that the Hastinapura chronology, which at best is tentative rather than definite, will be used to date many other excavations. It is impossible in a brief review to

go into all aspects of what is a very controversial subject, but some of the more obvious means whereby dates have been pushed back can be indicated.

Period IV, which contains a definite Gupta terracotta and, moreover, nothing that can be dated earlier than middle 1st century, B.C., may with more reason be dated from 50 B.C. to A.D. 400. Then there is a break—the town was burnt down. But why one of a hundred years? Fifty would be ample. It is doubtful whether Period III lasted more than 250 years, as there is nothing to account for a longer duration, the dating of N.B.P. ware prior to 400 B.C. being highly debatable, and the terracotta figurines all being 2nd and late 3rd centuries B.C. Before Period III there is again a break following on the settlement being in part washed away by the Ganges. Here, because the dating of painted grey ware back to 1100 B.C. fits a theory of Vedic origins, an unnecessarily long gap of 200 years is postulated when 100 at the outside would suffice, N.B.P. ware being clearly an extension of painted grey ware with a new fashion of surface treatment. Lal's dates for the beginnings of Periods II and III are 1100 and 580, but there is as good a case, if not better, for substituting 700 and 350 B.C. It is right that such a divergence of opinion, by no means peculiar to the reviewer, should be stated, but this need not obscure the fact that this is a first-rate excavation report.

D. H. GORDON.

THE MATRIX OF EARLY INDIAN CIVILIZATION (Proto-historic Investigation). *By* B. B. LAL. *Ancient India*, No. 9, 1953. (Received February 1956.)

This short résumé of the proto-historic period deals with a cultural epoch about which there is still a great deal of doubt. Much that is set down here with an air of certainty, unavoidable because of its summary nature, can be interpreted differently. As one instance, the conclusion reached with regard to the Southern and Eastern polished stone axes, following Warman, is probably incorrect. The whole of the chronology is still speculative, and much reduced datings of the various cultures and industries could with considerable reason be substituted.

D. H. GORDON.

THE EXCAVATION OF A NEOLITHIC DWELLING, and a Bronze Age Cairn at Mount Pleasant Farm, Nottage, Glam. *By* H. N. SAVORY. *National Museum of Wales, Cardiff*, 1955. *pp.* 19 + 4 *figs.* + 2 *plates*. Price not given.

Beneath a nearly destroyed Bronze Age cairn near Porthcawl, Dr Savory had the good fortune to discover traces of a Neolithic house. It was sub-rectangular like houses at Knockadoon, Clegyr Boia, Haldon, Ronaldsway, Rinyo, and Skara Brae, but smaller than most (about 19 ft. × 8 ft.). Drystone walling, quarried probably three miles distant, was flanked by post-holes, the arrangement of which suggests a ridge-pole roof. There was evidence that the entrance was in the north-west long side; the hearth may have been in the north-east corner. Sherds of Neolithic A and B wares, which may have been late in Piggott's Late Neolithic phase, were found inside and outside the floor area, some in post-hole filling.

Two urns, one given an early date and the other a late one, are to be assigned to the overlying cairn.

HUMPHREY CASE.

INVENTARIA ARCHAEOLOGICA: GREAT BRITAIN, 1st SET: Grave Groups and Hoards of the British Bronze Age. *Edited by* C. F. C. HAWKES. 10 *cards*. *Garraway Ltd., London*, 1955. 10*s*.

INVENTARIA ARCHAEOLOGICA: Ages des Metaux, France: Fascicule 1. *By* R. Joffroy, R. Audin *and* R. Riquet. 12 *cards. De Sikkel, Anvers. B. fr.* 75.

Readers of a past review (Antiquity, xxviii, 176) will remember that the purpose of this excellent international series is to publish associated finds. Publications are in the form of folders of punched cards (about 8½ in. × 11 in.), with line drawings to scale on one side of the card, and descriptions and bibliographies on the other.

The first British folder presents Beaker culture grave-groups from Dorchester-on-Thames, Oxon, and Radley, Berks; a Wessex culture grave-group from Radley, Berks; and Bronze Age hoards from Crediton, Devon, Leopold Street and Burgesses' Meadow, Oxford, and Barton Bendish and Stoke Ferry, Norfolk. It is pleasant to have the chance of praising the editor's full and scholarly comments, with the elegant drawings by Mrs M. E. Cox of the Ashmolean Museum and the excellent service given by the publishers.

The first French folder deals with Hallstatt and La Tène culture grave-groups from the Cote d'Or (Ste-Colombe-sur-Seine, Essarois, Magny-Lambert, Prusly-sur-Ource, Montigny-sur-Aube) and the great Vénat hoard from Ste-Yrieix, Charente. Again it is pleasant to be a reviewer, when one has the task of welcoming a detailed and thoroughly usable publication of late prehistoric French antiquities. May one hope that fuller and more original drawings, and to some extent fuller descriptions, will raise future publications, in the French series to a still higher standard? Humphrey Case.

JAHRBUCH DES RÖMISCH-GERMANISCHEN ZENTRALMUSEUMS, MAINZ. 1 *Jahrgang*, 1954. *pp.* 263 + 22 *plates* + *numerous figs. and maps. DM.* 30.

This well-produced volume, dealing with a broad range of subject and with excellent illustrations, gives promise of an important periodical. The officials of the Museum and the contributors deserve praise.

Elizabeth Schiemann describes early plant remains found at the Neolithic Rossen culture settlement of Ur-Fulerum, near Essen. Grains of emmer (35 per cent) were less numerous than those of barley (60 per cent), which included naked four-row and hulled four- and six-row; fruit and pips of crab-apple were found, and seeds of various wild plants.

Ulrich Fischer reports a now destroyed fragment of a wooden vessel from a Bronze Age grave of Montelius Period II at Heerstadt, Weserwunde; it was decorated with small bronze nails and tin bosses, and can be reconstructed as similar to the well-known vessel from Store Høj. A long article by Ernst Sprockhof discusses the sun symbolism shown in decorative schemes principally of Periods IV and V; elements foreign to northern Europe centred around the swan, are connected with the Greek myths of Apollo and held to be of central European origin.

Georg Kossack describes and discusses at length types of harness and bridle-gear found in Hallstatt C Iron Age graves in Bavaria; the introduction of foreign types, of evidently Danubian origin, may imply technological and social changes. Wolfgang Kimmig and the late Walter Rest describe grave goods from a *Furstengrab* at Kapple am Rhein, which is to be dated about 525 B.C., apparently in Heuneburg Phase IV. Bracelets of sheet gold are discussed at length, and their ornamentation derived from Etruscan styles; the pendant from Jegensdorf, Bern, showing granulation technique, is described as native work.

Wilhelm Schleinmacher contributes a note on a preliminary study for a painting of Mercury and Bacchus found at Pompeii, and discusses the light it throws on the artist's technique. Gustav Behrens gives a factual and concise account of provincial Roman fibulae, illustrated mostly from west German collections, with notes on unfinished and repaired examples and moulds.

Raymond Lantier describes a Merovingian terracotta plaque from Gérsin, Puy-de-Dome, and argues that it represents Christ triumphant over the asp and basilisk. Ernst Gall discusses the Carolingian *Westwerke* with particular reference to Corvey Abbey.

The reviews include one by Garašanin of Milojčic's *Chronologie der jungeren Steinzeit Mittel-und-Südeuropas.* HUMPHREY CASE.

LOUGH GUR EXCAVATIONS: NEOLITHIC AND BRONZE AGE HOUSES ON KNOCKADOON. *By* SEÁN Ó RÍORDÁIN. Offprint from *Proc. Roy. Irish Academy*, LVI, C, 297–459, 1954. 42 *plates* + 55 *figs.* 25*s.*

Knockadoon is a rather rocky limestone hill bounded on three sides by Lough Gur, lying in an isolated group of low hills on the Limerick plain. Professor Ó'Ríordáin excavated areas of prehistoric settlement on its lower slopes intermittently between 1939 and 1949.

Vestiges of sub-rectangular and oval timber-framed houses were discovered. Some were quite sizeable well-planned dwellings; one interior was some thirty feet long. Others seem to have been little more than haphazard shelters. A highly important overlapping sequence of pottery types was revealed. Earliest was Class I, which belongs to the Western Neolithic family; it had plain and rather profusely decorated aspects. Next came Class II, with flat bases and little decoration (which included cord impressions); it is compared to Swiss Horgen ware. Finally came Beaker wares, not previously found in such quantity in Ireland, and Food Vessel wares.

Life seems to have depended predominantly on cattle farming; bones of swine and sheep or goat were rare, and of wild mammals, birds or fish very rare. No evidence of prehistoric agriculture was found, although certain rubbing stones may have been parts of querns. Bone points were made; slate and struck flint, chert and quartz were used, and igneous rock, including specimens from the distant Tievebulliagh or Rathlin Island quarries. Finery included lignite bracelets and beads of stone, bone and glass. A socketed axe and some fragmentary bronzes were found; fragments of stone and clay moulds and of clay crucibles illustrate Middle Bronze Age technology. A few objects were found of the Early Iron Age, Early Christian and later periods, but no important structures.

The excavations were outstanding for the range of prehistoric material which was found. The report shows what devoted energy and practical common sense can achieve when applied to a small but well-chosen area. It is lavishly illustrated, has been meticulously prepared, and is in every way worthy of the great institution of whose publications it forms a part. HUMPHREY CASE.

FUNDKARTEN ZUR VORGESCHICHTE DER RHEINPROVINZ. *By* KURT TACKENBURG. *Rheinisches Landesmuseum, Bonn*; *Verein von Altertumsfreunder in Rheinlande*, 1954. *pp.* 107 + 1 *fig.* + 22 *maps in two colours. DM.* 9.50.

These maps show distributions of finds in the Rhineland belonging to periods ranging from the Lower Palaeolithic to late La Tène, superimposed on surface geology (solid *and* drift). They are clearly drawn, although rather small (1/1,200,000). Anyone who has attempted mapping of this kind will admire the author's devotion wholeheartedly and wonder at the checking and re-checking which must have been needed. More than 2,000 finds have been plotted and the bibliography covers over 70 pages. A short and useful summary is given of the major distribution patterns.

One hopes that the importance of this unpretentious work will not be overlooked. Here we often speak vaguely of the relationship between early settlement and surface geology; we have little scholarly documentation to show it. HUMPHREY CASE.

ARCHÄOLOGIE II: DIE ARCHÄOLOGENSPRACHE, DIE ANTIKEN REPRODUKTIONEN. *By* A. RUMPF. *Sammlung Göschen Band* 539, *Berlin*, 1956. *pp.* 136 + 12 *plates*. *DM.* 2.40; English price 4*s.* 6*d.*

This is probably the best archaeological value for money to be found in a long while. Rumpf has an unrivalled reputation in Greek and Roman archaeology, and it is fortunate that it is a scholar of his calibre who was chosen to write these brief handbooks on the basic principles and background of classical archaeology. Volume I dealt with the history of the subject, and the new volume covers the idiosyncrasies of the archaeologist's idiom, the materials used in antiquity and the copies of works of art executed in antiquity. No strictures here on keeping the sides of your trench clean, for Rumpf begins at the point where the ' dirt archaeologist ' stops and beyond which he can seldom follow. Of wider importance and well deserving the attention of scholars in other fields, archaeological or otherwise, is his enumeration of the cankers of modern scholarship—books outrageous in size or price, paper-mongering writers more interested in acreage printed than in saying anything new or useful, the love of saying everything in a way which leads to the comprehension of nothing, the spate of periodicals and repetition of illustration, the *ius primae noctis* which excavators and museum directors feel they should enjoy with their material even after it has well passed nubility. It is satisfying to note that in this review of the shortcomings of modern practice the one scholar quoted as an outstanding exception is an Englishman, but one wonders how many offenders there are!

JOHN BOARDMAN.

THE PYLOS TABLETS: texts of the inscriptions found 1939–54. *Edited by* EMMETT L. BENNETT, Jr., *with a foreword by* CARL W. BLEGEN. *Princeton University Press for University of Cincinnati* (*London*: *Cumberlege*), 1955. *pp.* xxxii + 252. £2.

A general account of Greek records in the Minoan Script was given by John Chadwick in ANTIQUITY, XXVII (1953), 196 f. Bennett now republishes the texts of the tablets found in 1939 with new readings and new joins, and adds the texts found in 1952–4. Blegen describes their actual discovery and the methods used by the palace to preserve and check them. The total number of these 13th-century records is now 1,199, and rather under half of them are new material. Many of the new tablets belong to categories already known, but make the picture more detailed and more precise, e.g. the records of women, of agricultural produce, and of smiths. Many, however, are new and some of these should be mentioned. A series of five tablets (one old and four new) gives the disposition of troops for coastal defence. Textiles appear for the first time in Pylos (tablets of the same general form are known in Knossos). Chariot wheels are listed in quantity, and corslets with the glosses *to-ra-ke* = *thorakes* and *ko-ru* = *korys* (helmet). The most spectacular gains, however, are the lists of tables, chairs, and furniture inlaid with decoration in ivory and other precious substances (see now M. Ventris in *Eranos*, 53 (1955), 109 f.). What could be better than ' footstool inlaid in ivory with a man and a horse and an octopus and a griffin ' (Ta 722)?

The book consists of three main parts: (*a*) Drawings of the tablets ' made by tracing in Indian ink the lines of the stylus as they appeared ' on photographic prints, which were then bleached; these show the exact layout of the texts; (*b*) the texts in normalized Mycenaean characters; where these conflict with the characters reproduced from the bleached photographs the readings are more trustworthy than those of the bleached photographs because they depend on autopsy, sometimes by more than one pair of eyes; (*c*) an index of all the words with their values according to Ventris, prefixed by Ventris'

table of equivalents. Many who have been working on the 1939 tablets and the considerable literature on the subject, which has accumulated since Ventris' discovery, will regret that Bennett has renumbered all the 1939 tablets, but the new volume is so handy and complete that it would be foolish as well as ungrateful not to adopt the new numeration.

T. B. L. Webster,
University College,
London.

ART AND LITERATURE IN FOURTH-CENTURY ATHENS. *By* T. B. L. Webster. *University of London*, 1956. *pp.* xvi + 160 + 16 *plates*. 25*s*.

Art, literature and philosophy both influence and are influenced by the temper of the age in which they appear. We might expect, then, these influences to have some common factors which can be distinguished. Believing this to be true, Webster looks at 4th-century Athens and discerns, in the philosophers' attitudes to the arts, in the arts themselves, and in current literary tastes, three successive attitudes to art and thought which are as well reflected by the three philosophers Plato, Aristotle and Theophrastus, as by their contemporary playwrights, orators, sculptors and vase-painters. The three attitudes he describes as ' seeing in contrasts, seeing the structure, and seeing the appearance '. The material to work from is varied but incomplete. Nevertheless Webster makes for the most part a plausible synthesis and incidentally gives an interesting picture of this most restless period in Greek history.

John Boardman.

THE MONAGRILLO CULTURE OF PANAMA. *By* Gordon R. Willey and Charles R. McGimsey, with an appendix on Archaeological Marine Shells by Robert E. Greengo. (*Papers of the Peabody Museum of Archaeology and Ethnology, Harvard University*, xlix, No. 2.) *Cambridge, Mass.*, 1954. *pp.* xiii + 158, 12 *tables*, 34 *figs.*, 20 *plates* (*also called figs.*). $4.65.

Until recently the archaeology of the Isthmus of Panamá had little time depth. The cultures of Chiriquí and Coclé, with their rich graves containing striking painted pottery, gold and other treasures, were known to date from the last centuries before the Spanish Conquest. Nothing earlier was known, in spite of the strategic position of the Isthmus, through which all land migrations between North and South America must have passed. By their discovery and investigation of the Monagrillo Culture, Prof. Willey and his associates have shown that this situation is due to imperfect exploration, and their work gives promise of future discoveries.

The Monagrillo Culture is found in shell mounds near the shore of Parita Bay on the Pacific Coast of the Isthmus, near the area later occupied by the Coclé Culture. The people appear to have lived mainly on shell-fish, supplemented by fish, turtles, and deer and other local land animals. There is no evidence about agriculture or vegetable foods. Their artefacts consist of simple pottery and stone implements. The pottery is chiefly in the form of bowls of various forms, showing irregular control of oxygen in firing, smoothed not burnished, and ornamented only by incision and very simple red painting. The stone implements are mainly pebble choppers and grinders with no shaping apart from that resulting from use, plus a few boulders with hollowed surfaces which served as metates, and some crude percussion-flaked projectile points and scrapers.

Another cultural type on a similar level, with a more limited distribution, is the Sarigua. Its pottery is still simple, but is better made and thinner than Monagrillo types.

Its chronological position is uncertain, but it seems to be either contemporary with the latter part of the Monagrillo or later than it.

The absolute age of these cultures is uncertain. They were found underlying various pottery types which seem to be contemporary with the rich culture of Coclé, but the interval which doubtless exists between them cannot be estimated. Their very simplicity makes it difficult to point to parallels elsewhere. The most suggestive similarities are between the incised curvilinear designs on some Monagrillo sherds and those on pottery of the Formative Period in Guatemala, but it is quite possible that Monagrillo is a later, backward culture.

The excavation was a most meticulous piece of work, and the report shows that all possible information was extracted from the data. It will doubtless be followed up in order to fill the gaps which remain in the sequence. The Museum is to be commended for printing its plates on paper which does them justice, unlike some of its contemporaries.

G. H. S. Bushnell.

THE ROYAL CEMETERIES OF KUSH—VOL. II: NURI. *By* Dows Dunham. *Museum of Fine Arts, Boston, Massachusetts,* 1955. *pp.* xxvii + 300, *with* 141 *plates and* 216 *figures.* $32.50.

The publication by Dows Dunham of the series of excavations carried out by Reisner in the Sudan some thirty to forty years ago marches splendidly on. Over a number of years, Reisner excavated all the known royal tombs of the Kushite rulers of Napata and Meroe, but the material has remained largely unknown except to those fortunate enough to be able to visit the museums of Boston and Khartoum. The lack of publication has been a serious hindrance to all workers on the history of the first six centuries B.C. and the first three A.D. in the Sudan.

Dunham, who was one of Reisner's collaborators, is now making the most handsome amends, and in a series of magnificent volumes is publishing in the fullest possible way the material which will enable us to reconstruct the history and to reassess the dating proposed by Reisner. We have already had volumes on the earliest Kushite royal cemetery at Kurru, and on the decorated scenes from the pyramid chapels at Begarawiya (Meroe). Now in volume II, published after volume III, comes the full description of the group of pyramids at Nuri. This group, which in date comes after the Kurru cemetery, begins with the burial of Taharqa and contains the remains of eighty-two pyramids, the burial places of kings and their queens. The range of date is from Taharqa, who died in 664 B.C., down to Nastasen, who is assumed to have died in about 308 B.C. It thus covers a length of time at a vital period in the history of the country, beginning with the ending of Napatan (25th Dynasty) rule in Egypt and covering the period when the capital of Kush was transferred from Napata to Meroe. The date of this event still remains in some doubt, but the final publication of these royal burials will give us all the information we are likely to have, unless excavation is ever recommenced on the town site at Meroe.

The book itself is sumptuous and the material is very fully published. It is presented in the form of a catalogue of the pyramids and associated tombs, beginning with a description of the structure and continuing with a list of the objects found, all profusely illustrated both by drawing and photography. As only too often, all the tombs had been robbed in ancient times. From the evidence of intrusive pottery this robbing was mainly Christian, as also happened at the not far distant group of burial mounds at Tanqasi. In spite of this, sufficient material remains for a great many of these pyramids to be identified with a named personage and for enough typological evidence to be gleaned to permit of their chronological arrangement. The topography of the site also gives help in making this

arrangement since it is reasonable to suppose that the most obviously favoured site would be occupied by the first comer, as indeed it is by the pyramid of Taharqa, and that each new burial would take place on the best remaining site. The typological and topographical evidence closely agree.

As well as these tombs of the Kushite kings, the remains of a church were found. It is of interest as being an addition to the small number of stone-built churches in the Sudan. It is surprisingly similar to and only very slightly smaller than the one at Ghazali, some fifteen miles away. Exception might be taken to the author's use of the term ' Coptic ' to describe the Christian material. Whatever the theology of the Nubian Christians at the time, the word ' Coptic ', with its national and linguistic connotations, is liable to mislead.

Comment on the significance of the pyramids and their associated material is kept for a later volume, so it is not profitable, until the author's views are more explicitly stated, to discuss what can be learnt. This work is a presentation of material which is essential for every worker in this field; it is unfortunate that few will be able to afford it.

P. L. SHINNIE.

MISSION DE RAS SHAMRA. Tome vi. *Le palais royal d'Ugarit publié sous la direction de* CLAUDE F-A SCHAEFFER . . . III *Textes accadiens et hourrites des archives est, ouest et centrales par* JEAN NOUGAYROL *avec des études de* G. BOYER *et* E. LAROCHE. Two volumes containing *text* (*pp.* xliii + 340) and *plates* (109). *Klincksieck, Paris,* 1955. *Dutch guild.* 70.40.

The present volume begins with a general survey by M. Claude F-A Schaeffer of the archives of the palace of Ugarit, which are edited by Prof. J. Nougayrol in the main body of the text.

These archives consist of clay-tablets from three points in the vast complex of buildings excavated at Râs-al-Šamrah, the ancient Ugarit, close by Mînat alBêdâ on the Syrian coast; they are named by the excavators the *Archives Ouest*, *Est* and *Sud* and the *Archives Centrales*. Excellent plans enable the reader to identify the offices from which the texts come; there are also photographs of the ruins and of selected tablets, enabling the expert to reach an independent judgment on such problems as the form of the tablets and the type of script. The present work deals with the collections found in the *Archives Ouest* and *Est* and to some extent with those in the *Archives Centrales*, which have not yet been fully explored; those in the *Archives Sud* are hardly touched.

The classes of documents in these various *Archives* varies somewhat: those in the *Ouest* are mostly administrative documents relating to the royal estates, e.g. contributions in labour or kind (those due from places in the hills being less than those from villages in the plains), and some letters; those in the *Est* relate to the capital city and its immediate suburbs, being mostly lists of persons resident there or in the neighbourhood, of contributions in kind and of stores laid up in a number of places, sales and receipts, and so on; those in the *Centrales*, as so far published, are chiefly legal documents, e.g. deeds recording ownership of property and registering changes of ownership for various reasons between citizens. In addition to these there are a few miscellaneous tablets in each of the three *Archives* such as letters and one scholastic text (witness to the presence of a student-scribe on the archivial staff). Almost all these documents 254 in number, are written in Babylonian syllabic cuneiform, not in the local Ugaritic alphabetic cuneiform script; and a very few are in the Hurrian (Horite) language. The script is middle-Babylonian with a few peculiarities in the form and use of certain signs; the language is Babylonian, fairly correct though marked occasionally by grammatical lapses. The period, for which the names of twelve kings have now come to light (two already known from the tablets found

at Tall-alAmarna over half a century ago), is from the 18th to the 13th century B.C.; and seventeen scribes sign the documents. One of the most interesting points is that the seals on the royal acts all bear the same design at the top of the *recto* without regard to the identity of the reigning king; this is the *hommage au roi déifié*, which is a well-known theme of Babylonian glyptic art (goddess and beardless young man at the left and seated king at the right side). This suggests that the successive kings, if they were not actually the lineal descendants of the founders of the dynasty (for one lasting over six centuries would be surprising in the ancient East) at any rate claimed to be such for political reasons.

The documents, as already said, deal with a variety of subjects; and several may be singled out for special mention for their human interest. The writer of one says that no *habirù* (the strange people first found in the letters from Tall-alAmarna and connected by some scholars with the Biblical ' Hebrews ') are present or settled in his district (no. 16.03). Another records how the king of Ugarit resolved to fetch ' the daughter of the great lady ' from the hands of the king of *Amurru* (an Amorite king) and how the girl, when she presented herself before him in the company of his servants, ' laughed continuously ' to express her delight (no. 16.270). A third deals with the case of several rascals who got themselves into trouble for forging ' the great seal of the king ' and were banished (no. 16.249). Others deal with the *trousseau* of a bride or gifts from the king to prominent subjects who are expected to make and do make a compensatory gift to him in return. The variety of subjects, in fact, is wide; and the wealth which they suggest, especially in precious metals, is very considerable.

There is a valuable discussion of the legal documents by Mr G. Boyer, drawing attention to the resemblances to and differences from Babylonian in the Ugaritic practice; the most noticeable is perhaps that the well-known *tirhâtu(m)*, which at Babylon is the sum paid by the bridegroom to his future father-in-law, is here the dowry which a bride takes with her to her new home. Other interesting peculiarities concern the intervention of the public authority in private rights, questions of fiefs and sureties, and certain points of personal law; but the field is too narrow for extensive comparison with other systems of law.

Mr Laroche publishes the few Hurrian tablets included in these archives.

The cuneiform texts are beautifully copied, carefully transliterated and translated, by Mr Nougayrol and furnished with adequate glossaries of Accadian technical terms and titles and a list of the ideograms used. Misprints and other slips are commendably few (e.g. AE for EA on p. xxxvii 1.1; p. 37 no. 15.81 11.7, 14 *it* for *it*, and no. 16/287 11.8–9 *ṭì* and *ṭum* for *tì* and *tum*; p. 77 no. 16/142 1.10 *ú-ba* for *ú-bal*). Unfortunately the referencing is as complicated as it is inconvenient.

Apart from these small points the whole work is impeccably edited and admirably produced and contains matter of the first importance for the study of early Semitic civilization.

MENSA SACRA: THE ROUND TABLE AND THE HOLY GRAIL. *By* A. A. BARB. Reprinted from *Journ. Warburg and Courtauld Institutes*, XIX, 40–67, 1956.

The author of this interesting study argues that the story of the Holy Grail is Christian in origin and that the pagan and mythological elements are accretions woven in by the story-tellers to please their secular, aristocratic audiences. This is a breath of fresh air and common sense on a subject that has too long been the sport of theorists, whose real interest lay in fertility rites or solar myths. Philologically, ' grail ' is the late Latin *gradalis*, a dish; the author shows how at the date of the earliest versions it was connected with the

dish (*de illo catino sive paropside*) used in the Last Supper. Iconographically a form of altar, with a series of depressions surrounding the central hollow, ' appeared ' in South France in the 10th century. The form is early Christian and the central hollow might be a round, a semicircle, or, more rarely, a rectangle; the type also influenced the design of the paten at the period when the story of the Grail was evolving. Considering the number of wooden altars that were replaced with stone under the influence of Carolingian reforms, it is questionable whether ' appeared ' should not read ' survived ', but this is a minor point. The author's further contention that the development with the ' national king ' (Arthur or Charlemagne) taking ' the place of Christ at the round table ' is equally convincing, though rather crudely expressed. He might have added a reference to Ottonian representations of the Court of Heaven on the model of the Imperial court, which find their counterpart in the Saxon description of the Apostles as thegns of the Lord (Andreas 11, 1–5). Some of the speculations on symbolism seem rather too elaborate, and we must confess to serious doubts as to any connection with Glastonbury; the whole association of St David with the site is suspect.

C. A. R. R.

PRÉHISTOIRE DE L'AFRIQUE, Tome I, Le Maghreb. *By* R. VAUFREY. Publications de l'Institut des Hautes Études de Tunis, vol. IV. *Librairie Masson et Cie., Paris*, 1955. *pp.* 458, 216 *figures*, 58 *halftone plates.*

Mr Vaufrey's book will be welcomed by all who are studying the prehistory of the West-Mediterranean, especially by those who want to know what happened on the coastal border of North Africa and in its hinterland. We must, however, say that for Mr Vaufrey ' prehistory ' here comprises only the Palaeolithic and Neolithic periods.

In the introduction (p. 9) Mr Vaufrey explains that he does not want to give only a general review of his conclusions about prehistoric North Africa, but something like a critically composed catalogue of the most important sites. On this basis he tries to prove his hypothesis, of which the essential element is that the Upper Palaeolithic did not spread from North Africa to Europe but vice versa. Though the author clearly states the hypothetical character of this theory, it will sometimes seem to the reader that it has influenced his choice and assessment of material.

Very important is the description of the geography of North-West Africa (pp. 9–13), especially for those who do not know the country personally. Interesting is also the history of investigation (pp. 14–21) which, together with an extensive bibliography (pp. 419–35) and numerous notes, will help to a more intensive study of the details. The reader will miss here, however, a record of the recent work done by archaeologists in the Spanish zone of Morocco, where investigation began later. It should at least have been mentioned—if only to give complete information—in the same way as the international zone of Tangier was mentioned. L. Pericot García's book (*Historia de Marueccos I, Prehistoria, 1ª parte: El paleolitico y epipaleolitico*, Tetuan, 1953) was published only when Mr Vaufrey's book was in print, so that he could not have known it. But the *Prehistoria del Norte de Africa y del Sáhara Español*, Barcelona, 1946, by M. Almagro Basch, could have been quoted; and if he wished to use only original work, there were the papers of M. Tarradell and J. Garriga Pujol (*El Paleolitico del Rio Martín*, Tetuan, 1951, and ' Observaciones sobro ol Pleistoceno de Marruecos ' (*Bol. R. Soc. Esp. H. Nat., sc. Geol.*, vol. 2, 48, 1949)), and also C. F. Posac Mon, ' Industrias liticas en el Marruecos oriental ' (*Cuad. de Historia Primitiva*, vol. 2, 1947); by the same author, ' Dos nuevos yacimientos liticos en el Marruecos Español ' (*Cuad. de Hist. Prim.*, vol. 3, 1948) and ' Yacimiento del Rio Nano en el Marruecos Español ' (*Cuad. de Hist. Prim.*, vol. 4, 1949).

Chapter 3 deals with the Lower and Middle Palaeolithic (pp. 21–125). The author

presents the sites in a geographical order, beginning with the most important ones. Critically referring to the different opinions and comparing the stratigraphical evidence of all of them, he arrives at better-founded results than a generalization of evidence of one or the other site could have given. This method, moreover, makes all the doubtful cases clearly visible. In this chapter Mr Vaufrey gives more credit to the readers' knowledge than in the next ones concerning the Upper Palaeolithic (chs. 4 and 5, pp. 127–289). Here he shows the development of the attempts to solve the Capsian problem, in order to make his special point of view understandable.

In the same way one could have wished to find an introduction to the Atérian problem. But only the question of the ' bifacial ' flint implements is discussed, and their relation to other groups. As they seem to appear only in a restricted area in North-West Africa, Mr Vaufrey prefers the theory of their relation with the Spanish ' Solutréen ' (Parpallo) and not with the Still Bay culture. Apart from chronological difficulties quoted by the author, it does not seem clear which routes could have been followed, as the one group, containing ' bifaces ' and situated more to the East, the ' Sbaikien ', is gravely attacked by Mr Vaufrey. It seems to be useful to compare Mr Pericot's view on this point, which he explained in the book mentioned above.

Where Mr Vaufrey separates the ' Capsien typique ' from the ' Capsien supérieur ', the modern methods of investigating the Palaeolithic period become evident, since neither the isolated type nor the technique applied allow one to decide to which group a site should be joined, but only the whole pattern of elements. So it seems correct to separate an ' Ibéro-Maurusien ' and an ' Inter-Gétulo-Néolithique ' as groups, which existed beside and contemporary with the ' Capsien supérieur '. We would also accept the author's conclusion that the ' Capsien supérieur ' is limited to an inland territory.

It is not so easy to agree with the author's opinion about the Neolithic of North-West Africa (pp. 291–368; ch. 6). He judges it from a point of view common to specialists in Palaeolithic subjects, and gives more value to flint implements than to pottery and other elements of culture. Remembering this, one may understand his point when he refers to the whole Neolithic complex as ' Néolithique de tradition capsienne '. This tradition is, exclusively, the tradition of the ' Capsien supérieur ', in the culture pattern of which the regularly trimmed trapezes are predecessors of the transversal cutting Neolithic arrow-head. Since the trapeze is unknown, the ' Ibéro-Maurusien ' does not seem to have been one of the ancestors of the North-West African Neolithic. But as the ' Ibéro-Maurusien ' is the one group of the Upper Palaeolithic distributed especially along the Mediterranean coasts, one has to ask what lines the very clear relations between North-West Africa and Spain in the early Neolithic period could possibly have followed; otherwise we would have to suppose that the Spanish Neolithic civilization began much later than the African. Could it not be that a Tardenoisian tradition and a Capsian one met in the West Mediterranean? It is really only the flint industry which can give a hint that Africa and Europe were related, while the pottery belongs to a culture that invades Africa from the East and the North and which did not necessarily come to Spain via North Africa. If we accept Mr Vaufrey's opinion we are bound to believe that, starting from the rather limited area of the ' Capsien typique ', the ' Capsien supérieur ' occupied a more extensive territory until the ' Néolithique de tradition Capsienne ' was distributed over all North-West Africa and Spain. But this cannot be proved. It can only be said that there is a strong resemblance in the flint industry of both countries and that they received identical Neolithic influences.

The pottery is introduced by the author as typically African, connected only with the Spanish pottery of El Garcel, Cueva de la Sarsa, etc. One would have expected him to mention its relations to the big group of the ' Impressed pottery ' (' Céramique imprimée '),

well known from the whole coast of the West Mediterranean and surely not invented by the people with Capsian tradition. If the Capsian peoples had made a pottery of their own one would expect that they would have continued with the same decoration which they developed on their ostrich-eggshell bottles, common already in the period of the ' Capsien typique '. If we admit that the characteristic Neolithic elements were introduced to the Capsian we have to be very cautious in order not to give too much value to relations which are based on one group of the elements only. In any case we cannot say more than that there might have existed a similar background.

We have to say furthermore that there are other ways in which Neolithic influences could have spread from the Eastern centres to the West. The typical Spanish arrow-head of the more recent Neolithic and Eneolithic period is a ' bifacial ' flaked one, known by different types. The same types are frequent in the ' Néolithique de tradition Capsienne ', but more in the East (Tunis) than in the West (Algeria) and almost lacking in the North-Western corner of North Africa. Could they not represent, also in the Neolithic of North Africa, a more recent development ? Are we not compelled to believe that they arrived in Spain in a different way, since they are lacking especially in those places where we should expect them to be more frequent ? There may have been a connection through the interior of North Africa where influences from Egypt could have progressed more slowly than others that came by sea. Besides the arrow-heads we may mention some motifs of the rock engravings, e.g. the ram with the sun-disc on his head, the ' roi-scorpion ' and others.

That the ' Néolithique de tradition Capsienne ' survived down to very recent times is stated clearly enough by Mr Vaufrey. In my opinion he should have explained more thoroughly that the ' vase carenée ' from Dar es Soltan is in fact a bell-beaker of the rather low form which A. de Castillo is accustomed to call ' cazuela '. Ruhlmann, who published it, did not believe that it was a bell-beaker because he considered the level where it was found to be much older. But the recent excavation in the cave of Gar Cahal (*Tamuda*, vol. 2, 1954, 355; *Germania*, vol. 33, 1955, 13) proves that the bell-beaker did come to Africa, and in earlier times the undecorated pottery of the Almeria type, invading a Neolithic culture that seems to recall the ' Ibéro-Maurusien '. Taking this as proven, one might ask whether the decoration made with ' comb ' (cf. pl. 51, 52) is really of earlier date than the same decoration on the bell-beakers, in which case it would belong to its predecessors, or if this decoration is only an imitation of that of the bell-beakers.

It would be very important to answer these questions in order to find out what was the rôle North Africa played in the distribution and progress of the neolithization. The Capsian culture as a whole—even as explained by Mr Vaufrey—seems to be one of ' food-gatherers ', and the ' Néolithique de tradition Capsienne ' does not appear to have made much progress on the way to ' food-producing '. We have some proofs of cattle-breeding and farming, though of the latter only by a few querns and sickle-flints, but nothing like to the same extent as in the first Neolithic cultures of Europe. So, to us, it seems more probable that relations between North Africa and Spain existed in the pre-Neolithic (or rather ' pre-ceramic ') period of the ' Capsien supérieur '—and this would explain the resemblance of the flint implements—and not during the full Neolithic time.

We have to thank Mr Vaufrey for having given us in his book a basis for all these thoughts and theories. The material is now easily accessible, and one has not to search for it in small local reviews. We must thank him furthermore for helping to intensify the studies in West Mediterranean and, at the same time, West European prehistoric archaeology. The numerous illustrations, very well drawn, and the great number of photographs will contribute to a better understanding of the subject, especially since the author has presented much unpublished material.

EDWARD SANGMEISTER.

FEDERATION MUSEUMS JOURNAL, Vols. I and II (New Series), 1954–55. *Edited by* G. DE G. SIEVEKING; *published by the Museums Department, Federation of Malaya; printed at the University Press, Cambridge.* $9 *net.* (21*s.*)

We offer the usual hearty welcome to this virtually new publication which is a resuscitation in a slightly more specialized form of the *Journal of the Federated Malay States Museums* (vols. I to XIX, 1905–1939). Both in format and contents, it achieves a high standard which we hope will be maintained. The new series thus begun ' will be reserved for papers in Ethnology, Anthropology and Archaeology ', a wise reservation, for the attempt to cover in a single journal the whole field of Nature and Man could never be satisfactory. This (double) number contains 143 pages, nearly half being devoted to a report of excavations at Gua Cha by the editor. Here was a rock-shelter containing graves and representing ' two phases of the Malayan Neolithic culture '. In the absence of any imported objects that might have provided synchronisms, no absolute dates are possible, but it seems that the second Neolithic phase was not long before the Christian Era. At another site beaked adzes like those of Gua Cha were found with ' a black pottery vessel believed to be imported from Greece in the third or fourth century B.C.' We have heard of this alleged Greek vessel before, and it is high time that it and the relevant facts about its discovery were published, together with a picture of it. Mr Sieveking's report is full and detailed, with plans, sections and many really beautiful drawings of the pots. We have no room even to mention the five other articles, all of them valuable.

The format, though pleasing, could be improved in details. The cover has (as it should) the list of contents but no date, place or publishers' address. This number is said to be for 1954–5, but it was received by us at the very end of 1956. It is a good thing to follow the practice (adopted by *L'Anthropologie*) of giving also on the cover the actual date of publication. Improvements could be effected in the printing of the plates, e.g., ' Walker, Plate I ' has two small pictures on one side and nothing on the other. It would have been better, and no more expensive, to have used *enlarged* photographs and printed one on each side. There are two other blank (i.e., wasted) plate-pages. These are details, however; the general effect is excellent, and we wish the new journal every success in the future.

O.G.S.C.

ANCIENT NEAR EASTERN TEXTS RELATING TO THE OLD TESTAMENT. *Edited by* JAMES B. PRITCHARD. *Second Edition, corrected and enlarged. Princeton Univ. Press,* 1955. [*O.U.P.*] £7.

We have already received the companion volume of illustrations (ANTIQUITY, XXX, 1956, 128). This one actually far outruns its title, for it is in effect a manual of translations of most of the major documents discovered by archaeologists during the last hundred years. ' Many of the relevant texts have been hitherto accessible only in obscure and highly technical journals ', and some of them are now out of date. Some have not been translated before.

The texts are printed under ten headings, and the names of those responsible for the translations include those of most of the leading students of Near Eastern languages, and are a guarantee of impeccable scholarship. There are 544 pages, ending with a useful list of contents arranged under the headings of language and an index of names. The editing of such a volume must have involved months of hard work, and it is beyond praise. Though indispensable for students, however specialized, it is a book that any layman can browse in with profit and enjoyment. Here are voices from the past speaking directly to us in our own language.

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